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VOL. XX. JULY—DECEMBER. **1849.**

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THE

RETROSPECT OF MEDICINE:

BEING

A HALF-YEARLY JOURNAL,

CONTAINING A RETROSPECTIVE VIEW OF EVERY DISCOVERY AND PRACTICAL IMPROVEMENT IN THE MEDICAL SCIENCES.

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EDITED BY

W. BRAITHWAITE,

LECTURER ON OBSTETRIC MEDICINE AT THE LEEDS SCHOOL OF MEDICINE, ETC.

VOL. XX. JULY—DECEMBER.

1849.

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ERRATA.

At page 16, line 1, for "*Induction*" read "*Inunction.*"
,, 448, line 5 from bottom, for "431" read "437."
,, 449, line 25, for "431-2" read "437-8."

HOSPITAL.

PRACTICAL MEDICINE,

&c., &c.

DISEASES AFFECTING THE SYSTEM GENERALLY.

Art. 1.—ON THE MODE OF ACTION OF MORBID POISONS.

By Dr. J. W. Tripe.

[The mode of action of those morbid poisons which (as in the case of the exanthemata) protect the system from a second recurrence of the disease, is a very interesting subject of enquiry. It is presumed that such poisons act by producing some permanent alteration in the blood or the tissues generally, or in both of these. Dr. Tripe proposes to ascertain in what this alteration or modification consists, and how it is effected, by a consideration of the laws which regulate the action of morbid poisons generally. He says:]

Dr. Robert Williams in his elaborate work on morbid poisons was the first clearly to point out the analogy between the action of the ordinary and of the morbid poisons." The laws which he pointed out are the three following:—"Firstly, that all poisons have certain definite and specific actions; secondly, that they lie latent in the system a certain definite but varying period of time before these actions are set up; and lastly, that the phenomena resulting from the poison when roused into action, vary according to the dose or predisposition of the patient. These laws are common to all poisons or classes of poisons. To these laws we may add a fourth and fifth, viz., that the specific action of poisons depends on the affinity existing between the liquids and the solids of certain parts of our organism, and the poison ingested; and fifthly, that this specific action ceases on the elimination of the poison from the system. The first law is one almost universally acknowledged, and forms the basis of the remedial part of the physician's art; for unless medicines had definite and specific actions, they would be valueless as remedies. As instances, we may adduce hydrocyanic acid, strychnia, castor oil, &c., which are severally known to produce, when taken into the fabric, certain determinate and invariable effects. It has long been admitted, that certain medicines—poisons—act more or less invariably on certain membranes or organs, or on

certain sets of membranes or organs; but that the action depends on an affinity between the elementary tissues, or part of them, of the organ so acted on, and the poison itself, is less universally allowed, although it is, nevertheless, equally as true. In poisons of this class are included antimony, arsenic, copper, strychnia, and many others. In proof of this position we may adduce the experiments of Blake, as quoted in the *British and Foreign Medico-Chirurgical Review*, (July, 1848, p. 181-9.) He states that his experiments warrant the following conclusions:—"The various substances—mineral medicines—may be arranged into groups according to the similarity or dissimilarity of their effects, each group being distinguished by re-actions not to be found in any other class, whilst the reaction of each of its members closely resemble each other. The classes thus formed agree with those which are adopted by chemists in their arrangements of the different elements according to their isomorphous relations; and the only conclusion to be drawn from this interesting coincidence is, that the physiological action of these substances depends upon some property they possess in connexion with their isomorphous relations." Thus potash and ammonia produce analogous effects; strontia, baryta, and lead; soda and silver; lime, magnesia, zinc, copper, manganese, nickel, and cadmium; platinum, palladium, iridium, and osmium; phosphorus, arsenic, and antimony; selenium and sulphur; and lastly, chlorine, iodine, and bromine, form separate classes, each of which have the most striking analogy in their physiological actions. The metallic salts in general; the salts of potash and baryta, arsenic and phosphoric acids, agree in destroying the action of the heart; the greater part of the acids, and of the salts of silver increase its irritability. The doses however, of these substances necessary to produce their specific effects vary considerably.

The second law, that all poisons lie latent in the system for a definite but varying period of time, admits of but one exception, and that is more apparent than real, inasmuch a although comparatively the action is instantaneous, yet a certain short interval elapses between the taking of hydrocyanic acid and its effects. Narcotics, purgatives, diuretics, and other classes of medicines, all occupy a period of time of longer or of shorter duration, according to the condition of the patient as regards plethora, state of constitution, &c., and also according to the greater or lesser time ordinarily occupied by each medicine in producing its effects.

As an example of the third law, Dr. Williams adduces the action of oxalic acid. If this poison be taken in a small dose, death ordinarily results from inflammation of the stomach; whilst if the dose be very large, death may happen in a few minutes from the shock to the nervous system. We here have a good example of the modification induced by a variation in the dose; tartar emetic may be mentioned as another example.

The fourth law, that the specific action of poisons depends on the affinity existing between the liquids and solids of certain parts of our organism, and the poison injected, does not admit of so ready

a proof as the other laws. The following considerations may be adduced in favour of it:—firstly, that the ordinary action of a medicine may be induced on the distant organ, or organs, whenever it finds its way into the fabric, whether it had been introduced by the stomach, or injected into the blood. The experiments of Dr. Blake, already related, afford strong confirmation of this statement. The well-known fact, that particular purgatives will act on definite parts of the alimentary canal, that diuretics act on the kidneys, mercury on the liver, iron on the blood, all prove the same. Additional proof is also afforded by particular poisons attacking only particular viscera or tissues; and by their being found in greater quantity in certain organs, or tissues, as copper in the lungs, arsenic in the liver, &c., and in certain secretions, as sulphuric acid in the urine.

The results of the disease caused by the introduction of the morbid poisons into the blood tend to the same end. Mr. Paget, in his Lectures on Nutrition, &c. (published in the *Medical Gazette*), adduces the Essays on Symmetrical Diseases, which were written by himself and Dr. Budd, as evidence that all diseases in which the two corresponding portions of the opposite sides of the body are similarly diseased, depend on the circulation of a morbid material in the blood, and an identity of composition in the tissues affected. He then proves, that certain morbid poisons, or other morbid matters, have certain "seats of election." Thus the syphilitic poison attacks in some patients corresponding portions of the tibia and of the skull with great exactness. The pelves of two lions were exhibited, in which new osseous matter had been deposited, by a rheumatic disease, producing an irregular kind of patern, both sides being precisely alike, thus proving beyond a doubt that the specific action of the morbid poison must have resulted from some affinity between it and the tissues attacked.

That the specific action of the poison ceases on its elimination from the system, cannot be very readily determined, as we cannot easily ascertain when the whole of the poison has passed out. Perhaps the best and most certain method of determining this point, is to examine chemically the urine and other secretions—excretions—to ascertain for how long a period the poison is passing off. But although this has been, and may be readily done in the case of mineral poisons, yet it cannot be applied to the class of morbid poisons, as we have no physical or chemical means of determining their existence. The only organic poison that we can prove to become mixed with the blood, and to pass out of it again—the amanita muscaria—affords very strong evidence of the truth of this law. The following quotation from Dr. Pereira's Materia Medica, first edit., Vol. I., p. 15, supports this, "A man, for example, may have intoxicated himself to-day by eating some of the fungus; by the next morning he will have slept himself sober; but by drinking a teacupful of his urine he will become as powerfully intoxicated as on the preceding day." Can more powerful evidence be afforded of the entrance of the morbid poison into the blood, the circulation

of it through the system, the production of its peculiar effects on the brain, the elimination of the poison through the agency of the kidney, and the consequent cessation of its specific action.

In opposition to this view of the subject, it may be objected that the specific effects of lead, or of arsenic, will frequently continue for weeks, or perhaps for months, after the poison was taken into the system, and that we have no right to assume that this action was caused by the actual presence of the poison. It has, however, been shown, that arsenic may continue to be excreted from the body for as long a period as five weeks after the dose was taken; and I knew of a person who excreted lead by his skin for more than three months after he had ceased to be exposed to the influence of the poison. Most, if not all of the specific effects of a morbid poison may apparently cease before the whole of the poison is eliminated from the system. Thus a person may infect another with the exanthematous disease from which he has suffered, after all the ordinary specific effects of the poison have ceased; that is to say, after the eruptive, and sometimes even after the desquamative stage has passed by.

Dr. Williams further states, "that the general laws observable in the actions of morbid poisons are for the most part precisely similar to those which govern medicinal substances, or only differ in a few minor points. These poisons have their specific actions, their periods of latency, while their phenomena equally vary according to the dose, or the predisposition of the patient." That the poisons are specific is proved by the precision of their course, and of the pathological phenomena which each individually exhibit. Morbid poisons, also, have their periods of latency, the duration of the period varying in the different diseases induced by them. Thus, the poison of scarlet fever ordinarily lies latent from a few hours to ten days; of measles from ten to fourteen days; and of small-pox from sixteen to twenty days.

The chief distinctions between the morbid and the mineral poisons consist, first, in the power which the former have of increasing within the system to a comparatively indefinite extent; and, secondly, in their specific effects being independent of the amount of the virus absorbed, a small dose producing as certain and as severe effects as a large one. Many of them, also, are capable of infecting persons at considerable distances, and of exhausting the susceptibility of the membrane diseased to any future attack. These peculiarities are possessed by the virus of scarlet fever, and will, therefore, be briefly considered.

The theory of the increase of the virus of morbid poison within the system was propounded by the ancient physicians; for instance, Hippocrates attributed the phenomena of fever to a morbid matter in the blood, which being *concocted* for some days, was rendered fit for expulsion from the body. This theory has been revived and more philosophically explained by Liebig, who ascribes the peculiarities of this class of diseases to the fermentation of the morbid poison in the blood. Liebig's doctrine of fermentation is, that fer-

ment or yeast is nothing but vegetable fibrine, albumen, or casein in a state of decomposition, with their particles in a state of intestine motion. This motion being communicated to the particles of the vegetable, or of the animal matters with which the ferment is in contact, will, other circumstances being favourable, communicate the same kind of intestine movement to them, and thus split up their particles into new and less complex compounds. The gluten, or other azotised matters in the fluid, act therefore the part of fresh ferment. Liebig elsewhere states, that animal fibrine, albumen, or casein may act as a ferment as well as vegetable. The poison of scarlatina having passed into the blood in perhaps an almost unappreciable quantity, multiplies itself within the body in a manner analogous to a ferment, until the whole of the pabulum in the blood that enables it thus to multiply itself, is consumed. Should there be no pabulum in the blood, no reproduction of the poison could ensue, nor could any of the ordinary effects of scarlatina occur, unless the quantity of the virus absorbed was sufficiently great to attack any membrane that had not been previously affected, or in which the change previously made had been effaced by the tendency existing in all altered diseased tissues to revert to their normal state. The nonsusceptibility of the patient to the subsequent action of the virus, is ordinarily attributed to this modification of the blood; but for reasons, to be hereafter stated, I do not consider this to be the sole cause of it. The second distinction between the mineral and vegetable and the morbid poisons, namely, that, the effects of the latter do not depend to any great extent on the dose of the poison, follows as a necessary result of the preceding law; for, if the amount of the virus circulating in the blood is ordinarily in proportion to the quantity of matter in the blood fit for its reproduction, it follows necessarily that the resulting disease will depend, not so much on the dose of the poison, as on the amount of pabulum then existing in the blood for its increase.

Medical Times, Sept. 8, 1849, *p.* 196.

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2 —ON THE CAUSES OF INTERMITTENT AND REMITTENT FEVERS.

By Dr. Gayley.

Dr. Gayley has arrived at the following conclusions regarding the Etiology of Intermittent and Remittent fevers :—

1. That the lungs and the liver are the great decarbonizing organs of the body; that, their function being complementary, the activity of that function is always in an inverse ratio.

2. That during winter, the lungs, from the part they perform in the generation of animal heat, are the more active organs. On the other hand, in summer, the liver is the more active.

3. That exposure to a low temperature repels the blood from the surface to the internal organs. If this exposure takes place in win-

ter, the lungs being then the active organs, the brunt of the congestion falls on some part of the respiratory apparatus; hence we have bronchitis, pleurisy, pneumonia, and catarrh, as the prevailing diseases.

4. But if this exposure takes place in summer, the liver being then in a state of stimulation, the force of the congestion falls on it. It becomes deranged, involving all those organs, more or less, whose blood has to pass through the liver to reach the heart. Hence, we have bilious affections, as intermittents, remittents, dysenteries, &c., as the prevailing diseases.

5. That the pulmonary diseases of winter and of cold climates, and the hepatic diseases of summer and of warm climates, are both produced by the same agents acting on the system; the different effects being solely owing to the different modifications of the agents, and the different conditions of the system when exposed to their influence; and we might with as much truth say that malaria was the cause of the one as of the other.

Nature is simple in all her operations. It is only in proportion to our ignorance that she appears mysterious. The mystery that all admit to hang over the origin of these diseases, and the failure that has hitherto attended every effort to explain it satisfactorily, we think is solely owing to the spirit of enquiry being on the wrong track. The idea of a specific poison, first started by Lancisi, has been embraced by most writers on the subject ever since; as a consequence, their labours have been expended in tracing the origin of a thing, the very existence of which is hypothetical.

Of the truth of this, the Proteus-like appearance that this agent assumes in the hands, or rather the heads of different writers on the subject, is *primâ facie* evidence. Thus one describes it as being softened or entirely disarmed by passing over 800 yards of water. (Sir G. Blane.) Another thinks it can safely perform the voyage from Holland to England, yea, even to Scotland, a distance of not less than four hundred miles. (M'Culloch.) It cannot ascend to the second story of a house, and yet can seize its victim on a mountain side four hundred feet high. (Ferguson.) Stygian-like it ascends from the bowels of the earth, and angel-like descends with the dews from heaven. Were it delineated with all its eccentricities attached, I doubt much if even its most devoted admirers would be pleased with the picture.—*American Journal of Medical Sciences*, 1849.

Medical Gazette, Sep. 7, 1849, *p.* 437.

3.—*On the Treatment of Intermittent Fever by Tartar Emetic.*—By — Moore, Esq., Assistant Surgeon, Upper India. [Mr. Moore has treated a great many cases of intermittent fever in one of the Indian regimental hospitals, and he gives it as the result of his experience, that tartar emetic is preferable to any other medicine hitherto employed in this disease. He says:—]

Tartar emetic has been administered in nauseating doses, prior to the first stage of the fever, or the stage of invasion. It has been

continued during the second or hot stage,—the stage of excitement. It has been continued during the third or sweating stage,—the period of the fever's crisis and decline.

The exhibition of tartar emetic in the treatment of intermittent fevers has been deferred until the bowels have been freely purged by pills or powders of calomel and jalap, followed by the common mixture of senna and Epsom salts. This preparatory evacuation of the bowels by calomel, jalap, or by some other equally active purgatives, has been invariably observed. It is a standing order in the regimental hospital.

When it has been ascertained that the intermittent fever is not complicated with inflammation of the viscera of the thorax and abdomen, the lancet, leeches, and the cupping instrument, are kept in reserve until such inflammations become developed.

These preliminary steps taken, the administration of tartar emetic is commenced after the first paroxysm of the fever has clearly declared itself; the proportionate dose of the medicine for each patient during the progress of the fever being regulated by mixtures marked A, B, C, D, E, of which an ounce every half hour or every hour is a dose. A contains onegrain of tartar emetic to one-hundred ounces of water. B one grain to fifty ounces. C one grain to twenty ounces. D one grain to ten ounces. E one grain to five ounces.

The advantages to be derived from regulating the doses of tartar emetic in this manner consist in having the means at hand to keep the patient's system under the influence of the tartar emetic, without producing any violent effects on the stomach and bowels either by vomiting or purging.

The chief object to be kept in view is, to prostrate the patient's strength so completely, that when the first stage of the fever, or the stage of invasion, has commenced, it must work on the patient's system, debilitated by the nauseating doses of the tartar emetic. In like manner, the prostration of the patient's system is kept up during the progress of the second and third stages of the fever.

The value of tartar emetic as a remedial agent in uncomplicated intermittent fevers consists in anticipating the fever by prostrating the patient's system previous to the first stage, or the stage of invasion, setting in. In this respect tartrate of antimony and potash possesses advantages over all other medicines classed as emetics. The patient's system once brought under the influence of the medicine, can be kept in a state of extreme prostration by the administration of decimal and centesimal subdivisions of a grain repeated at short intervals without the effects of vomiting and purging being produced.

The centesimal subdivision of one grain of tartar emetic is more suited for children and weakly females than adult males. On this account the mixture has been introduced into the list. When so small a quantity of tartar emetic fails in producing any effect on the system, mixture B, or the fiftieth part of a grain of tartar emetic, can be substituted. The mixture most frequently used in

the regimental hospital of the 5th infantry is that marked D in the list, or the decimal subdivision of a grain repeated every hour.

The quantity of the tartar emetic can be increased or diminished by changing the mixture, and by shortening or lengthening the intervals of time at which each dose is given.

In three-fourths of the patients thus treated with tartar emetic, the attacks of intermittent fever, if not cut short at once, have been modified. Except in very obstinate cases, each recurring paroxysm has exhibited fewer marks of the acute form of the disease; and in point of duration, each stage of the fever has undergone a modification. In the simple uncomplicated form of intermittent fever, blood-letting has never been prescribed, nor has bark in any form been ordered. In cases of intermittent fever complicated with congestion and inflammation of one or more of the internal viscera, the lancet, leeches, and cupping have been freely resorted to in order to aid the effects of the tartar emetic.

Occasionally it happens that if the duration of the fever—quotidian, tertian, quartan—be protracted, the patient's system becomes habituated to the tartar emetic. A mixture in which half a grain of tartar emetic has been ordered every hour has failed to produce the effect of prostration, vomiting, purging, or sweating.

Under these circumstances it is useless to persist in prescribing tartar emetic. The medicine must be omitted. The loss of its influence in prostrating the patient's strength, and in arresting the fever during its different stages, is but temporary. Twelve or fifteen leeches ought to be applied to the epigastrium and over the liver; or, the quick abstraction of fifteen ounces of blood from the epigastric and hypochondriac regions, by means of two or three cupping glasses applied at the same time, should be resorted to. This mode of local depletion is preferable to the application of leeches. Calomel and antimonial powder, five grains of each; or, calomel and James's powder, in the same quantities, given at bedtime; and on the following morning a purgative draught, or purgative mixture, into which compound jalap powder enters, may be prescribed every fourth hour, until free evacuations from the bowels have been produced.

Further treatment of the disease is suspended until a fresh paroxysm of the fever has distinctly declared itself. The tartar emetic is then resumed, and prescribed in the decimal subdivisions of a grain. as in mixture D. This temporary suspension of the tartar emetic, and change in the treatment, has generally succeeded in producing favourable results.

Tartar emetic is a medicine well worthy of an extensive trial in the treatment of intermittent fever. Unaided, it has seldom played false in subduing the uncomplicated form of intermittent fever, however severe. Aided by the lancet, by cupping, or by leeches—aided also by mercurial purgatives—it has seldom failed in its duty, to either patient or physician, in cases of intermittent fever complicated with congestion and inflammation of one or more of the internal viscera.

I do not recommend tartar emetic as an *anti-periodic.* So frequently have I been deceived by the anti-periodic virtues said to exist in a certain class of medicines, that I confess myself a sceptic as to any such qualities being inherent in medicine. Quinine, bark in powder, and bark in decoction, administered in small and large, and frequently in repeated doses, have failed to check the onset of the fever, or to modify its symptoms. A single dose of Epsom salts has converted a quotidian into a tertian fever; a tertian into the quartan type. Under such circumstances, to which of these medicines ought we to attribute the virtues of anti-periodicity? But to the value of tartar emetic as a *remedial agent* in the treatment of intermittent fever, the results of more than 300 cases discharged cured, from the Regimental Hospital, 5th Infantry, Gwalior contingent; the results, also, of cases treated under my orders, as Civil Surgeon in charge of the district of Kuchwahargar, in 1847; and at present in charge of the district of Chundeeree, bear sufficient evidence.

Medical Gazette, Sep. 14, 1849. *p.* 445.

4.—*On the Treatment of Typhus.*—By Dr. Jones Lamprey, Trim, County Meath.—[In an account of the epidemic fever as it recently occurred under his observation in Ireland, Dr. Lamprey speaks thus of that form of the disease which presented the characters of our Typhus:]

The treatment of these cases, which lasted for such a lengthened period, was necessarily modified and altered according to circumstances, but was principally determined by the condition of the pulse, as in the typhoid form, which denoted the use of stimulants or the reverse. Port wine was largely used when delirious restlessness was present. The antimonial treatment originated by Dr. Graves answered in restoring the nervous system to a quiet and composed state.

Where the petechiæ were of a very dark colour, or threatening to run into a putrid condition, I have found the exhibition of yeast, in combination with camphor and nitric ether, of great service in staying the septic process, besides acting as a powerful stimulant.

I cannot speak too highly of the stimulating and antiseptic properties of this mixture. I have seen it remove the dark, livid hue of the skin in this form of fever within a few hours; and, as another proof of its antiseptic powers, I have found that, on administering it in cases of dysentery attended with great fetor of the dejecta, it speedily removed all odour, and, at the same time, rather counteracted the frequency of the discharges from the bowels. The following is the formula I used:

℞. Cerevisiæ fermenti, ℥ x. Camphoræ, ʒ ss. Ætheris Nitrici, ʒ iv. Misce.

An ounce to be taken every hour, or every second or third hour.

The mixture soon becomes so agreeable to the palates of the patients, that it is necessary to keep it concealed from them, for fear of their making "too free" a use of it.

Dublin Quarterly Journal, August, 1849, *p.* 79.

5.—*On the Treatment of the Diarrhœa and Dysentery of Fever.*—By Dr. Hudson, Navan, County Meath.—[In the diarrhœa and dysentery of the second stage of fever, Dr. Hudson found that the internal use of mercury did no good. He thought that the application of mercurial ointment over the cœcum was sometimes useful; and so, in some cases were leeching and blistering in the same region, with the internal use of mucilage and sedatives. Dr. Hudson remarks:]

I very soon experienced the inefficacy of the mineral astringents, acetate of lead, nitrate of silver, and sulphate of copper, whether exhibited by the mouth or by enema. No astringent had so good an effect in my hands as oil of turpentine, in doses of six or eight minims, given in emulsion.

Several medical friends who, like myself, were disappointed with the mineral astringents, tried the turpentine at my suggestion, and all report favourably of it. One of them, who had charge of a large temporary hospital, informed me that in some tedious cases he found advantage from changing the form of exhibition, and giving Chio turpentine in pills.

Dublin Quarterly Journal, August, 1849, *p.* 73.

6.—*How to distinguish Typhoid Rash from Flea-bites.*—By Dr. Lamprey.—In addition to the "central punctum," which has been described by Dr. Lalor as a useful diagnostic in determining whether the maculæ were those of flea-bites or of fever, I have found that if the shirts of the patients presented the appearance of blood dots, it was conclusive that the "maculæ" were caused by fleas. The reason of this is obvious, and a knowledge of the fact has often determined the nature of the eruption where the puncta could not be found. *Dublin Quarterly Journal, August*, 1849, *p.* 81.

7.—*How Epidemics displace one another.*—One epidemic often follows close on, or drives out, another. When plague prevails in the East, the announcement of the appearance of small-pox is hailed with joy, as the harbinger of the disappearance of the former. At Mons, an epidemic of small-pox and sweating sickness has expelled (?) the cholera. Some persons have been attacked by small-pox, others by the sweating sickness: the latter not being attended with danger; the former often fatal.

In Poland, also, the plague has expelled the sweating sickness and small-pox, which had superseded the cholera. Here the sweating sickness presented all the horrors by which it was characterized in the north of Europe at the end of the fifteenth century.—*La Presse Médicale.*

Medical Gazette, May 25, 1849, *p.* 893.

8.—*On the Nature of Inflammation.*—By Dr. A. Billing, F.R.S.—The contraction of the heart is *muscular;* of the arteries, *elastic.* The heart *contracts* and relaxes *alternately.* The arteries keep up a constant *contractile pressure* on their contents; not, as has been commonly supposed, an alternate contraction and relaxation, but a continued contractile effort, both longitudinally and transversely, which is overcome by the action of the heart: when there is much blood sent into them, they are distended; and if there be little blood sent into them, as after hemorrhage, their tendency to contract causes them to close, so as to keep always full, and to preserve a continuous stream of blood, even during the temporary relaxation of the heart; and the arteries yielding, and adapting themselves to the pressure of the heart, and recontracting on their contents, whilst the heart is relaxed and filling, is the cause of the equability of the stream in the veins.

It is necessary here to state distinctly my opinion respecting action, as depending on the nerves. I consider that the *muscles* and *capillary arteries*, though *differing* in *tissue*, have each inherent in their structures a faculty of contracting, *organic contractility; this contractility* being *acted upon* by the *nervous influence*, the *result* is *contraction*, the nervous influence being discharged into them from the nerves.

A variety of circumstances leads us to the conclusion that the nervous influence is analogous to, or depending upon, if not identical with, the electrical principle, or fluid, whatever that be.

It is very common to say, that in inflammation there is an increase of arterial action; but a consideration of the phenomena, and of the nature of arterial action, will show that in inflamed parts the capillary arteries are weaker in their action; that there is diminished arterial action, for the action of arteries is contraction: now the arteries in inflamed parts are evidently larger than before—less contracted; that is, acting less.

The way to diminish inflammation is by increasing the action of the arteries, as by cold or astringents, which make the arteries contract,—that is, increase their action; so that, so far from the arteries in an inflamed part being in a state of increased action, one means of *diminishing inflammation* is by increasing arterial *action* in the part inflamed. It is common to remark the *throbbing* of the carotid arteries as increased action; but the more they throb, it shows that they the *more yield* to the injecting force of the heart.

As the heart therefore acts against the capillaries, if we cannot cause them to contract strongly enough to resist its force, we are obliged to diminish the force of the circulation, either by taking away blood, which decreases both the quantity of blood sent to the arteries and the action of the heart itself, and in this way we leave less for the arteries of the inflamed part to do; or we can lower the force of the heart by medicines, such as digitalis, &c.

Medical Gazette, Aug. 31, 1849, *p.* 380.

9.—*On Congestion.*—By Dr. C. J. B. Williams.—Congestion is defined by Dr. Williams to be local hyperæmia with retarded motion. Its chief causes may be classed under two heads: 1. *Those of venous obstruction;* and, 2. *Those of atony of the vessels* (capillaries and veins). Among the first kind of causes, are mechanical pressure, cold, efforts of straining, valvular disease of the heart, tubercles in the lungs, &c. A numerous class of causes is comprehended under the head of atony of the vessels. In some causes the atony is general, as in extreme debility and adynamic fevers; and the distinctive character of the congestion is to be hypostatic. It may be local; arising from over-distension from long continuance in one position, or even from congestion itself produced by some mechanical cause; from the *intropulsive* effect of cold, or malarious and epidemic influences; or from over excitement of the vessels; short of that degree which is necessary to produce inflammation, from sudden arrest or diminution of the proper secretion of any organ; though it is not here always easy to say whether the congestion be the cause or effect, and either is removed by the remedies employed for the other. The production of congestion by over-excitement is exemplified in several familiar instances, and may be traced under the microscope.

When a slight irritant, as a weak infusion of capsicum, is applied to the web of a frog, it first causes contraction of the vessels, especially the arteries, then quickly follows enlargement of the arteries and other vessels with very rapid motion: after a while, the vessels gradually contract and return to their natural size. But if the stimulant application be repeated several times, so as to prolong the determination of blood into the part, the vessels do not then uniformly contract. The arteries indeed shrink, but the capillaries and veins remain congested, and thus present completely the condition given in our definition, excess of blood with diminished motion. This dilated state of the capillaries and veins must be chiefly ascribed to their losing tone after excitement, more than the arteries; but the process I have been describing is accompanied by changes also within the vessels; numerous pale corpuscles adhere to the sides of the small vessels and contribute to impede the current and cause congestion by obstruction. Whenever the stimulus applied has been strong, this obstruction amounts to entire stagnation, and many vessels appear much enlarged, and filled with stagnant blood, or rather with an accumulation of red particles entangled in the coherent pale globules. For this reason the vessels in which the blood is stagnant, are of a deeper red than others, the red particles being arrested whilst the liquor sanguinis passes on.

London Journal of Medicine, August, 1849, *p.* 727.

10.—*On Some Points connected with Scarlatina.*—By Dr. P. Newbigging.—[During the epidemic of scarlatina which recently prevailed in Scotland, Dr. Newbigging had the opportunity of seeing

a considerable number of cases at "Watson's Institution" in which 120 children of both sexes resided. The treatment adopted by Dr. N. consisted, in the first instance, in the liberal administration of antimony, preceded by an aperient, and this repeated towards the disappearance of the eruption,—tepid sponging and careful regulation of the diet; and locally, the early and frequent application of lunar caustic, solid or in solution, to the throat, and the employment of water acidulated with nitric acid, as a gargle. When anasarca appeared as a sequela, a brisk purgative was given, and the administration of antimony and the antiphlogistic regimen were resumed; and in some cases leeches were applied over the region of the kidneys. Dr. Newbigging makes the following remarks in reference to the *condition of the urine in scarlatina:*]

The alleged connection between albuminous urine and degeneration of the kidney in this disease, is a subject which has excited considerable interest, and this association of cause and effect received an apparently strong corroboration from the result of Dr. Hamilton's observations on scarlet fever, as it occurred in his practice in and around Falkirk.

In the inference drawn from these observations as to the cause of dropsy, I for some time concurred. My original opinion, however, became modified by certain observations made in my practice generally, and my faith in its accuracy has undergone a still more material abatement in consequence of the investigations I have had an opportunity of prosecuting at Watson's Institution.

These have led me to consider the anasarcous condition, which arises during the progress of the convalescence from scarlet fever, to be, in very many cases, dependent on mere functional derangements, amenable to treatment, and leaving the health uninjured as regards organic alteration.

Confirmatory of this view, I may mention that in an early stage of the disease, even at the time the eruption was just declining, having instituted an examination of the urine, I ascertained that, in every case, the albuminous haze was distinctly present, and remarkably so during the progress of the desquamation. That this appearance occurred without alteration, either in the specific gravity, except to a very small extent, or in the quantity of the secretion; and that it continued from a period varying from four or five to ten days, gradually becoming less defined, and finally disappearing altogether.

This occurred in cases in which the urine maintained its normal standard as to quantity, where the patients were well, and certainly unaffected by any dropsical symptoms.

I have been encouraged to lay these observations before my professional friends, in consequence of the corroboration they seem to have received by the careful investigations made by Dr. James Begbie, and published in the "Monthly Journal" for January. His remark corresponds with my own experience, that "if careful examination of the urine be instituted, albumen in small amount will be found to exist in every case of scarlet fever."

It is worthy of remark that the quantity of urine voided at the period of desquamation is even greater than in health, although it may contain albumen, and this is interesting, as, in dropsy occurring, it may be subsequently in the same case, the amount is greatly diminished, sometimes altogether suppressed, so that any diminution of this secretion, without apparent increase of albumen, may be almost certainly considered as the forerunner of anasarca.

These observations, although corresponding with those of Martin Solon, who ascertained the presence of albumen in *twenty-two* out of *twenty-three* cases, are, I am aware, irreconcileable with those of some other authors, more especially with Philippe, who states that, although the succeeding dropsy was very common in the mild epidemic which he witnessed, yet, in *sixty* cases of the disease, in which he tested the urine for albumen, employing for this purpose both heat and nitric acid, no trace of that principle was discovered.

Dr. Scott Alison remarks in his "Clinical Observations," that he has observed dropsy to ensue more frequently after a severe attack of scarlatina, than when the symptoms were of a more moderate character. Also, that the urine in some cases according to his analysis, was of specific gravity 1·005, while in others it was as high as 1·030; and he is of opinion that the kidney becomes organically affected at an early period of the disease. From these deductions, it is almost unnecessary to remark, that my own investigations, as well as those of others, differ essentially. I am greatly disposed to believe that, in many instances, the albumen is eliminated at the kidney—not owing to any disorganisation of that viscus, but that the blood, in all eruptive diseases, undergoes certain changes, and that in scarlatina this is exhibited by the presence of albumen in the urine; and that, in cases where the kidney is organically affected, as, doubtless, it is occasionally, that this arises from a specific inflammation of that organ.

I am sensible that these observations are partial and incomplete, and had I again an opportunity of investigating a series of cases similarly circumstanced, I would avail myself of more minute analysis; at the same time I feel disposed to draw the following conclusions from what I have observed:—

1st. That in very many cases albumen appears in the urine at an early period in scarlatina, without other alteration of this secretion, or affection of the system generally.

2nd. The diminution in the quantity of this fluid may be considered as a certain forerunner of anasarca

3rd. That the treatment should be strictly antiphlogistic, the diet being for some time carefully regulated, so as to exclude much azotised food.

4th. That the patient, however mild his symptoms, should be confined to bed for several days after the eruption has declined.

[Dr. Newbigging made trial of *belladonna as a prophylactic* He says:]

Having ascertained the number of children unaffected with scarlet fever, or who were uncertain as to ever having had it—making, in all, sixty-nine—I directed that belladonna should be administered to them, in the proportion of one-sixth to one-fourth of a grain twice a-day, according to the age of each child—the first dose being given before breakfast, and the last dose at bed-time. This plan was adopted on the 16th October. Three new cases occurred between that and the 20th. After that date no child was affected, nor has there been any instance of scarlet fever since that period in the institution. The use of this preventive was continued during upwards of five weeks.

It may, perhaps, be asked why the belladonna was not administered at an earlier stage of the epidemic—indeed, on the first occurrence of a case of scarlatina? My answer to this apposite question, I confess, is not perfectly satisfactory, viz., want of faith, and hence confidence, in the prophylactic properties of belladonna, and this though aware of the report made at the Orphan Hospital of Langendorf, in Prussia, in a family of 160 individuals, where belladonna having been administered immediately on the occurrence of the epidemic, two only took the disease.

I should now consider it my duty to lose no time in making use of this medicine on the first appearance of this disease, and I would strongly recommend the same plan of practice to those of the profession who are connected with similar educational institutions, with the view, not merely of attempting to ward off a malady so uncertain in its progress, and occasionally so fatal in its termination, but also with the object of accumulating information on a point of such paramount importance to the public health.

The opinion I have adopted on this point has been greatly strengthened by a similarly beneficial result, produced some time afterwards in another case. I was requested to visit a young gentleman at a large educational seminary. I found him labouring under scarlet fever, with profuse eruption, an aphthous, and very painful condition of the throat, accompanied by all the usual symptoms exhibited in the acute stage of a smart attack of this disease. I caused my patient to be removed, a few hours after first seeing him, to the house of a relative, and placed his brother, who continued to reside in the seminary, upon belladonna. This treatment was adopted in the other members of the family, consisting of nineteen, who had not previously been affected with scarlet fever. No other case occurred.

I am well aware, and can fully appreciate, how much, at times, the practical physician may be disappointed, under apparently similar circumstances, as to the efficacy of an acknowledged, and it may be favourite, remedy; and doubtless it may fail, and we may be unable to explain the reason of its want of success; but these two instances of the apparent efficacy of belladonna, administered under circumstances in which a continuance of the disease might have been expected, weigh greatly with me in advocating a fair trial, at least, of this remedy.

Monthly Journal, September, 1849, *p.* 1046.

11.— *On the Treatment of Scarlatina by Induction.*—By Dr. SCHNEEMANN.—Translated by J. L. Milton, Esq.—[Dr. Schneemann, physician at the Hanoverian court, has published a work advocating a new mode of treating scarlatina, which, he says, materially shortens the duration of the disease, and checks all infection at the end of the third or fourth day. Mr. Milton gives the following translation of those portions of the pamphlet which relate to treatment:]

From the first day of the illness, and as soon as we are certain as to its nature, the patient must be rubbed every morning and evening over the whole body with a piece of bacon, in such a manner that, with the exception of the face (?) and hairy scalp, a covering of fat is everywhere applied. In order to make this rubbing-in somewhat easier, it is best to take a piece of bacon the size of the hand, choosing a part still armed with the rind, that we may have a firmer grasp. On the soft side of this piece, slits are to be made in various directions, in order to allow the oozing out of the fat: and this is still further promoted by placing the bacon, for some time previously to using it, near the stove,—in the oven, or on the hob. But it is not proper to make the friction with a warm piece, and the fat must be allowed to cool before using it.

The rubbing must be most conscientiously performed, and not too quickly, in order that the skin may be regularly saturated with fat. During the process, the body is never to be wholly uncovered, but only that part on which the operation is being performed, and, if considered necessary, the whole can be done under the bedclothes. But such precaution as this is unnecessary; the children may safely get up. No injury will ever be done by it, and after once seeing it done, the mother or nurse soon learns to relieve the medical man of his task.

Although this plan, from the mess it makes, is not calculated to find favour in the fine world—although it dirties bed and linen, as well as the persons of the children, yet the first few days of its application produce results which make all this forgotten, and inspire the mothers with enthusiasm for this method. With a rapidity bordering on magic, all, even the most painful symptoms of the disease are allayed; quiet, sleep, appetite, and good humour return, and there remains only the impatience to quit the sick room. To obtain this, however, other things are necessary besides mere infriction with fat, but still I think myself bound to impute the most important share of the merit to this peculiar treatment of the skin. The truth of this assertion will appear from a citation of the first results which follow on the treatment indicated.

1. The improbability, I might say impossibility, of the patient getting cold while the skin is thus covered with fat—a point in no disease more important than here; were this the sole benefit of the fatty infriction, it would still be of great service.

2. The dry brittleness of the skin, and the tormenting itching, are by it not only materially alleviated, but for the most part fully put a stop to. Every practitioner knows how often the itching and burning of the skin in scarlet fever are unendurable to children,

keeping them constantly in distressing movement, and robbing them of sleep. Hence, children are generally well satisfied with this rubbing-in, and often ask for a repetition of the proceeding long before the time is come.

3. The influence on the physiological functions of the skin is still more important. During the coming-on of scarlet fever, the skin becomes diseased, in consequence of which it dies off. During this illness, and until a new covering is again prepared for the surface the functions of the skin are naturally very faultily, or, during the desquamation, probably not at all, performed. In order to explain the extent and importance of the imperceptible functions of the skin, merely mechanically viewing the matter, I refer the reader to the accurate experiments of Seguin, which fix the quantity of matter thrown off from the outer skin at eleven grains per minute in a grown person, and therefore more than two pounds per day. What efforts must it cost the organism to lead so large a quantity into other paths, in order to throw it off when the skin is incapable of doing so! But the preservation of the whole being is the first and most important law, and however great the effort may be, the other organs must perform this function.

(I must here observe, that at first Dr. Schneemann thought—and indeed, in this portion of his pamphlet, stated—that no desquamation takes place under this treatment, so that, in fact, the functions of the skin are maintained in full integrity. Later observation, however, convinced him that, with all possible care, desquamation to a certain extent will ensue. This he communicated to me, requesting me to insert it in the translation. To resume:—)

4. With this disappearance of the desquamation disappear all those bad symptoms which attend on it. This feature alone of this method of treatment will have its weight with the practitioner in all ages.

In order now to give a striking proof of the importance and the bad influence which the interrupted functions of the skin produce on the healthy activity of relative, even if distant, organs, I may cite the fact, that death is always the result where more than one half of the skin has been destroyed by fire or boiling liquid. Sooner or later such is the case.

A similar destruction of the skin ensues in scarlet fever, with this difference, that it takes place gradually, and thereby the organism is better enabled, by employing all the activity of the body, to find aid against the mischief which, to the injury of the patient, must result from the cessation of the functions of the skin.

5. The oxidation of the blood is thus considerably promoted, the interrupting of which is the cause of such serious phenomena.

As the disease of the throat is not improbably in great part due to the interruption of the functions of the skin and lungs, it must naturally disappear, or not present itself, where these are continued in integrity; and such is found in practice to be the case.

6. Owing to the fatty covering, the skin is kept in a moist state, and the cuticle thrown off cannot be driven about the room by cur-

rents of air, as happens where the patient has been kept dry: and thus one fertile source of infection is kept closed up, it being well known that infection is most easily communicated at the period of desquamation. The author believes, not only that such is the case, but also that by this method the danger of infection under any circumstances is materially lessened with the disappearance of the eruption from the skin: inasmuch as the process of generating infectious material is interrupted by restoring the skin to its normal state, a point of which he need not point out the importance.

7. By shortening the period of desquamation, and protecting the patient against the sequela of the disease, the duration and treatment of this can be shortened to a period of six to ten days—an immense advantage to the patient and his relatives, particularly as he is always liable to relapses, and is, even in mild cases, considerably weakened.

8. The remedy is (cheap), harmless, practical. and is perhaps never counter-indicated.

The linen must not be too frequently changed, as a clean shirt takes up more of the fatty matter than one already saturated, and hence the skin is sooner deprived of its fatty covering.

The rubbing-in is to be kept up twice a day for three weeks, and once a day during the fourth.

The patient is, after this, to be daily washed with cool water and soap, and then only is the warm bath to be commenced.

This process does not, as has been argued in some colleges, interfere with the natural course of the malady, and thus expose the individual to second and third attacks.

In severe cases, the remedy may be repeated three or four times within the twenty-four hours. The main point is to keep the skin *always* cool and moist; and here, even with all possible precaution, the skin will sometimes come off in certain places. But cases of this kind are only unimportant.

The practitioner will do well to fix the exact hour at which the rubbing-in is to take place; it will then most probably be better and more regularly performed.

[Dr. Schneemann recommends also that the patient should be confined to bed as little as possible, should have light but not starving diet. and should not be kept hot. Dr. S. speaks highly of the use of *belladonna as a preventive.* He says,]

Its action appears to be, by altering the relations of the nervous system, to diminish the disposition to the disease. Thus, when it is moderate. the belladonna extinguishes it; and when high, diminishes it. Of this the author has in many instances practically convinced himself. In numerous instances, only one child of a family was attacked. His prescription is—Take of extract of belladonna, one to two grains; distilled water, one ounce. Mix. To each child are to be given, morning and evening, as many drops as it has years. It is to be continued at least fourteen days.

But not to confine himself to his own testimony, Dr. Schneemann

gives that of Jordens, Ettmuller, Hedenns, Gumpert, Hufeland, Martius, Pormey, Behr, Benedix, Thaer, &c., who have testified to the value of belladonna; and though, he says, as many opponents might be found, yet it is scarcely credible that all the former had deceived themselves. In certain sections of the circle of Bayeux, Dr. Feron preserved from scarlet fever all the children who had not been attacked before he commenced operations. In 400 cases near Valenciennes, treated with belladonna, not one person was attacked. Hahnemann has, however, only recommended it where the eruption is smooth, bright-red, and in large patches. But where there are roughness and purpleness of the skin, it seems to be useless; and it may be that the bad success of many cases was owing to the neglect of this distinction.

Lancet, Sept. 15, 1849, *p.* 285.

12.—*On the Treatment of Scarlatina by Hot-Water Applications.*—By F. A. Bulley, Esq., Surgeon to the Berkshire Hospital.—[Mr. Bulley describes a mode of treatment which he has adopted and found successful not only in scarlatina, but in every instance of febrile disturbance that has come under his observation within the last few years. He gives as an illustration the case of a farmer, who, after several days of premonitory symptoms, became covered with a rash. The next day Mr. Bulley was sent for and found his patient labouring under an attack of scarlatina, which he (Mr. B.) believed would prove a very severe one. The following treatment was adopted:]

The patient was placed upon a bed, on which, the sheets having been removed, two blankets had been laid, then a flannel pad, composed of four thicknesses of coarse household flannel, sixteen inches long by ten inches wide, stitched round the margin to keep it together, and wrung out of hot water till almost dry, was laid as hot as he could bear it upon the pit of the stomach and over the region of the heart; and while it was still warm, the blankets, one by one, were carefully folded round his body, so as to completely confine the perspiration and heat, which might be generated by this process; at the same time, another pad of two thicknesses only of the coarse flannel, wrung out of boiling vinegar and water, was placed round the throat for the relief of the local inflammation. After he had remained enveloped in the wrappings a little more than half an hour, during which time the febrile excitement had been somewhat increasing, he burst into a profuse and general perspiration, in which he was allowed to remain for two or three hours longer, with an evident abatement of the symptoms, when the blankets were carefully removed, and the flannel pad withdrawn; after this, he had some refreshing sleep, and but little return of the burning fever; he was now ordered to take half a drachm of Dr. Stevens's saline powder in a mixture every four hours, and to use the following gargle to the throat:—

℞. Armenian bole; burnt alum powdered, aa. gr. 40; brown sugar, gr. 60; water, ℥viij, Mix.

[This plan of treatment was so successful that on the seventh day from the first appearance of the eruption, the patient was going about his farm. Mr. Bulley makes the following remarks upon the treatment:]

I have had the satisfaction to observe, that the simple method which I have adopted in the treatment of fever, and which I practised in the foregoing case, has never, in any trial which I have made of it, failed in its object. I have now used it in a great number of cases, some of them of the worst description, and in every instance where the process has been judiciously managed, it has either at once annihilated the fever, mitigated its severity, or diminished its duration. I have also found it extremely useful in cases of small-pox, in constitutions where the circulating power has been naturally so feeble as to be insufficient of itself to throw out the morbid elements from the blood by the skin.

Medical Times, August 25, 1849, *p.* 153.

13.—*On the Dropsy following Scarlatina.*—By Dr. Hughes.—[At a meeting of the South London Medical Society, after Mr. Swete had read an account of cases of scarlatina,]

Dr. Hughes said, it is several years since I stated the fact, that it is in the milder forms of scarlatina we have usually anasarca, the effect of exposure to cold. The parents brought children to the hospital swollen all over, and at first the previous scarlatina was denied; but on close enquiry it came out that the child had sore throat, and sometimes an eruption. The cause of anasarca he believed to be a suppressed action of the skin, the consequence of exposure to cold; congestion of the kidneys supervened—perhaps it might be called an inflammatory action, for there was occasionally a passive hemorrhage from these organs. All Mr. Swete's cases were mild, and he thought that in all the anasarca cases the urine, if examined, would have been found albuminous. He considered it singular that the head affections should come on and no albumen in the urine; and he also thought that in an institution, such as Mr. Swete had the care of, it would not have been difficult to have prevented the convulsions by attending to the skin. It is more important to take care of the skin after a mild than after a severe attack of scarlatina; for in the latter the parents keep the child in the house. Mr. Hicks has asked, is the disease from an inflammatory or debilitating cause? It is not necessarily either; it may be inflammation of a low type, so as to be fatal in twenty-four hours. In most cases children are cured by antiphlogistic means. It was essential to order a flannel dress, and the occasional use of the warm bath. He had never seen the kidney of a child who actually died of the disease. In one case where the child

died after many months, the kidney was white and creamy: he did not think this singular, for the function of the kidney may be disturbed just as that of the skin. In the advanced stages he had seen benefit from a stimulant, especially the tr. fer. sesquichlor.

Medical Gazette, July 6, 1849, *p* 38.

14.—*On the Treatment of the Nephritic Dropsy which follows Scarlatina.*—By Dr. F. J. Behrend, Erlangen.—Dr. Behrend observes, that the first thing to be done is to diminish the congestion of the kidneys, so as to prevent the exudation of plastic matter, and to restore the diuresis. For this purpose the best means are repeated applications of a sufficient number of leeches—from six to ten—to the lumbar regions, or repeated cuppings: the former seem the best remedy. The second indication is to restrict the diet, and to administer mild diuretic salts, as the acetates of potash and soda, which act also on the intestinal canal. Infusion of elder flowers is useful as a drink, for promoting diuresis. The body should be kept warm by being wrapped in flannel, impregnated with the vapour of sugar: this is prepared by throwing pounded white sugar on glowing coals, and holding the flannel over it. The limbs may also be enveloped in raw wool. I have lately tried the effect of sponging the skin with turpentine, and then covering it with flannel; but cannot speak with certainty as to the efficacy of this treatment. If the dark colour of the urine return after it has been removed by means of local depletion, this must be repeated, without any regard to the apparent weakness of the child. This state of the urine is indicative of a highly congested state of the kidneys. If the attack be less acute—if there be œdema, and the urine be albuminous, but clear and in sufficient quantity, spirits of nitric ether may be used, in addition to the diuretic salts. A restricted diet is not so necessary, as that the child should be kept in bed and in a warm atmosphere, so that the skin may be kept in a state of functional activity. Friction with oil of turpentine, warm baths, and mild purgatives, are indicated: sanguineous depletion is not proper. The child must be kept very warm for a long time: it should be daily rubbed with flannel for a month; and if it continue pale and weakly, iron should be administered; and the patient should have the benefit of country air. In dropsy of the pericardium and pleura, and in hydrocephalus, there is so great danger, that these affections will have to be treated without reference to the original cause of the disease. There is but little hope of recovery, unless nature takes some extraordinary course.

London Journal of Medicine, August, 1849, *p.* 770.

15.—ON ANÆMIA.

By Dr. C. J. B. Williams.

[Among the causes of anæmia, are insufficient food, impure air, and various chronic diseases; but especially irregularity of the uterine functions.]

The general sypmptoms of *anæmia* are those of general muscular weakness, weakness of the heart, feebleness of the circulation, organic weakness, and imperfect sanguification. The surface also exhibits physical signs of the scantiness of the blood; and the venous murmurs heard in the neck, and the aortic murmur frequently heard, give additional evidence of its thinness. When drawn, the blood is thin and watery, and forms a very small contracted clot, generally with a buffy coat. The red particles are diminished more than the fibrin; and the albumen is generally defective in quantity, except in paraplegia. Symptoms indicating irritation or exaltation of function often occur, many of which are referable to the intestinal canal. Sometimes there are more direct signs of excitement of sensibility, of the excito-motory function, or even of the mind. Dr. Williams explains the fact of the nervous system being generally excited in anæmia, by the peculiar distribution of the circulation through the nervous centres. He adopts the view, that the quantity of blood in the head and spinal canal is subject to variation; the intra-cranial and intra-spinal vessels being unable, not being exposed to atmospheric pressure, to accommodate themselves to the reduction of the blood, and hence retaining more than their proper share. The effects vary, according to the propulsive power possessed by the heart. "Under the influence of temporary palpitation, fever, or other kind of excitement, the brain and spinal cord, through their uncontracted vessels, which are among the nearest to the heart, receive an unusual share of its exalted but partial force; an erethism of some one or more of the functions of these nervous centres is the consequence: and pain, spasm, sensorial excitement, intolerance of light and sound, or sympathetic irritations, of some kind or other, occur." There may be in such cases, obvious signs of an excess of blood in the head, while the extremities and surface are bloodless, and more or less cold. Epistaxis, if it occur, may produce momentary relief, but may ultimately add considerably to the evil. "On the other hand, if the heart's action is feeble, it may be inadequate to propel the blood accumulated in the vessels of the brain; it therefore stagnates, and may cause some of the symptoms of congestion in that organ. Hence headache and giddiness, relieved by the recumbent posture, drowsiness, impaired mental faculties, obscured vision and hearing, partial paralysis, and, in extreme cases, coma or catalepsy. In such cases the blood is accumulated more in the veins and sinuses of the brain than in its arteries; and, not receiving enough force from the heart to keep it in full motion, it partially stagnates, and the functions of the corresponding parts are impaired

in proportion. This congestion may be only temporary, and lead to no serious results; but in some cases I believe there occurs an event that has not been noticed by pathologists—namely, a coagulation of the blood in the sinuses, and a consequent permanent obstruction to the passage of the blood through the brain. I have met with several cases more or less corresponding with the following description:—A young female becomes anæmic, and after exhibiting various symptoms of feeble general circulation, with headache, drowsiness, and impaired sensorial functions, suddenly becomes worse; passes into a state of stupor, with dilated pupils, sometimes varied by slight manifestations of delirium, throbbing of the carotids, and partial heat of the head, and dies comatose. On opening the head, a small quantity of serum is found under the arachnoid and in the ventricles, sometimes with a little lymph (in one case there was none). The vascularity of the membranes is remarkable, but the vessels most distended are the veins, and in the larger of these and in the longitudinal sinus, there is a firm coagulum. In parts, especially at the torcular herophili, this coagulum blocks the whole sinus, and exhibits a separation of fibrin, portions of which are softened down into that opaque purilaginous matter which was long mistaken for pus, but which Mr. Gulliver has shown to be a mere disintegration of the fibrin, which mere stagnation in a warm temperature may effect. These have been taken for cases of meningitis. No doubt inflammation may supervene in them occasionally, but in two cases that have fallen under my notice, there was no adhesion of the arachnoid nor deposit upon it, nor any other unequivocal mark of inflammatory action; yet the fibrinous and bloody concretions in the veins and sinuses were most remarkable for their size and firmness. It appears to me most probable, that these affections originate in the encephalic congestion connected with anæmia. Fibrinous concretions form on the transverse bands of the sinuses, and increase until they considerably obstruct the passage of the blood: hence the impaired state of the cerebral functions, amounting at last to coma. Reaction may take place, with determination of blood, and even inflammation, and these cause those symptoms of partial excitement that sometimes exhibit themselves; but neither during life, nor on examination after death, are the proofs of excitement so prominent as those of obstruction and interruption to the cerebral functions. It must be remembered, that in anæmia the fibrin of the blood is not diminished in proportion to the other animal contents, and it has a greater tendency to coagulate than in healthy blood."

In the *treatment* of anæmia, "a nourishing diet, with as much animal food as the digestive powers of the patient can master—tonics that best restore the appetite, the powers of digestion, and sanguification—the use of means, if necessary, to promote the natural excretions—and an exposure of the patient to the pure air and light of heaven, as free and as long as the strength and sensibility will bear—form the chief items of the treatment." Dr.

Williams has a high opinion of the utility of iron, especially the syrup of the iodide, preceded, if necessary, by some mild vegetable tonic. Besides tonics, special symptoms require appropriate treatment. Diffusible stimulants are often required to obviate temporary weakness; sedatives and narcotics, to allay nervous excitement; external stimulants, rest in the horizontal posture, and warmth to the extremities, to counteract the unequal distribution of blood. Depletion is of questionable utility; and more good may be done by mild stimulants and tonics, with derivants. If bleeding be at all employed, it should be local, and moderate. The nervous symptoms do not always cease with the removal of the anæmia; in such cases nervous excitement should be avoided; and the most serviceable medicinal means are the metallic tonics; especially valerianate and sulphate of zinc, nitrate and oxide of silver, and sulphate of copper.

London Journal of Medicine, August, 1849, *p.* 725.

16.—*On Anæmia as a Consequence of Rheumatism.* — By Dr. O'Ferrall, Dublin. [In commenting on some cases of acute rheumatism at St. Vincent's Hospital,—]

Dr. O'Ferrall stated, that he was induced to believe that this disease had a tendency, in its latter stages, to produce phenomena connected with a diminution of the globules of the blood. Some modifications in the constitution of the blood, during rheumatic fever, had been already observed; it had been ascertained that, at an early period of the disease the fibrine is increased; but it would appear that, subsequently, there is, in many instances, a diminution of the colouring matter. The attention of Dr. O'Ferrall was first called to the subject by observing that, in cases with endocardiac complications, after the employment of depletion and mercury, a cardiac bruit of a different character from that which originally presented itself, continued to persist, notwithstanding the steady employment of the usual means for subduing inflammatory action. By-and-bye, cases terminating fatally came under his observation, in which, although this bruit was present to the last, no morbid appearances could be detected in the heart upon *post-mortem* examination. Afterwards, he found that a râle musicale in the cervical vessels very constantly accompanied this peculiar cardiac bruit; and he was led to suspect, that after the subjection of the inflammation by bleeding and mercury, an anæmic condition followed the use of these remedies, which would require a very different treatment for its subdual. He accordingly ordered chalybeates, as in an ordinary case of chlorosis, and found the cervical and cardiac bruits to disappear under the use of this remedy. As yet, he imagined the anæmia to be due to the effects of the antiphlogistic treatment employed in combating inflammation; but during the last few years he has had repeated opportunities of observing, that a râle musicale in the neck, and a bruit accompanying the first

sound over the aortic valves, very frequently coincide with the decline of rheumatic fever, in cases where no active and weakening remedies have been employed; and he has been thus led to the conclusion, that the natural tendency of the disease in its advanced stages is to produce a diminution of the hæmatosin of the blood, evincing itself by its ordinary physical phenomena. This observation is one of very great practical importance, teaching us the necessity of discriminating between those cardiac sounds produced by the participation of the heart in the general rheumatic disease, and those arising from deterioration of the blood. The treatment in the two cases must obviously be of a totally different nature.

Medical Times, Sept. 15, 1849, p. 217.

17.—*On the Employment of Manganese in Anæmic and other Affections.* By M. HANNON.—Manganese and iron are almost constantly found united in the same minerals, and can be separated with difficulty. Again, iron is not always efficacious in chlorosis, and fails in curing anæmia arising from cancers, from tubercles, from prolonged and abundant suppuration, &c. In these cases, it cannot be the iron that is deficient in the blood, but some other ingredient; and it is probable that iron is united to manganese in the blood; and that cases of anæmia, unsuccessfully treated by iron, might be cured by manganese. M. Hannon first tried the effects of this agent on himself. He took at first a grain of the carbonate of manganese daily, increasing the dose to four grains by the end of the first week, and to eight grains by the end of the second. At the end of a fortnight, he experienced symptoms of plethora; the appetite increased, the pulse became stronger, and the colour of the interior of the eyelids was heightened. He then administered manganese to some anæmic patients; some of them experienced nausea for two or three days, after which the medicine was tolerated. In a short time, its beneficial effects became manifest in the increase of colour, in the fuller and more frequent pulse, in the energetic movements, and general improvement of all the functions.

The presence of manganese in the blood was discovered by M. Millon, who presented a memoir on the subject to the Academie des Sciences of Paris. His observations have been confirmed by M. Hannon.

[Several illustrative cases are given. The first mentioned is one of extreme chlorosis, in which the patient was sent into country air, and took iron for some time, without benefit. We are told that]

The patient was then directed to take one of the following pills daily before breakfast, and another before dinner: Extract of cinchona, carbonate of manganese, of each a drachm. Mix and divide into four-grain pills. After she had used these pills for a fortnight, the cheeks and conjunctivæ regained their colour, and the swelling of the feet disappeared. The following pills were then ordered. Sulphate of manganese, carbonate of soda, of each a drachm; fresh

charcoal, honey, of each a sufficient quantity to make a mass, to be divided into four-grain pills. A fortnight after the employment of this medicine, the bellows-sound had disappeared; the pulsations of the heart were strong and loud; and an energetic impulse was felt on applying the hand. There was no syncope; and the appetite had returned. The dose of the pills was increased; and, a month after, menstruation occurred, and the patient became plump, and able to bear much exertion. She digested and slept well—in a word, was cured.

[Another case is that of a young lady affected with phthisis:]

Iron, with opium, was prescribed; but it increased the cough, and brought on obstinate constipation. Syrup of the phosphate of manganese was then given, with cod-liver oil; the latter being added rather to prevent the contact of air with the manganese, than from any expectation of its producing good effects The constipation ceased; and the cough became more bearable, and ceased in a fortnight. The patient then began to recover *embonpoint.* A month after the knuckles assumed a very remarkable brick-red colour, which has continued up to the present time,—a period of nearly a year and a half. This patient took three *gros* (216 grains) of phosphate of manganese, in doses of three grains daily.

Madame R. was affected with cancer of the uterus. She complained of remittent pain in the hypogastric region, and suffered much while at stool. In the evening, she was troubled with severe lancinating pains, which often continued through the night. She was excessively weak, and of a pale yellow hue. She was troubled with palpitation, and a *bruit* was heard in the carotid. The feet frequently swelled. Syrup of the iodide of manganese was given with syrup of horse-radish, for several months. The pains did not leave her, but the anæmic appearance completely disappeared. To calm the pains, opium, with extract of hemlock, was prescribed; and the patient became apparently cured.

Mademoiselle M., aged 14, of a scrofulous constitution, had glandular enlargements in the neck, ulceration of the transparent cornea of the left eye, and caries of the first phalangeal bone of the index finger of the right hand. Being the daughter of a peasant, she had lived exclusively on vegetable food; but was ordered to take meat, and to drink beer. Syrup of the iodide of manganese was given in doses of a spoonful two or three times a-day. Under the influence of this, and her improved diet, she became less lean; soon after, the cornea regained its transparency, having been washed with a lotion containing gr. ss. of nitrate of silver to an ounce of distilled water. The suppuration of the carious bone ceased, and the finger was cured.

M. G. B., aged 38, had been treated with mercury for some years, for constitutional syphilis. The bones were sound; the skin was affected with all kinds of eruptions; the tongue had long been the seat of an obstinate tumour; and there were syphilitic ophthalmia and iritis. Fumigation and iodide of potassium were persevered in for

several months, but without effect. Iodide of manganese was then given, with syrup of sarsaparilla; and in a month, the patient was completely healed. He was directed to continue the use of the manganese; and, as he has not since applied for relief, it is probable that he has had no relapse.

These cases have been selected from a number of similar ones, and shew the efficacy of the new remedy proposed. Manganese has in all cases produced a more rapid effect than iron, in cases of simple anæmia. In the forms of anæmia cited, all the cases had resisted iron, and all yielded to manganese. The other cases are respectively of phthisis, cancer, scrofula, and syphilis;—all inducing almost irremediable cachexia, and all rapidly alleviated by manganese. The effects of the manganese, as observed in one case (phthisis) are remarkable. Iron seldom produces a similar result; if it improves the state of the blood, it increases the cough; so much so, that many practitioners abstain from its use in phthisical cases. In all the scrofulous cases, the iodide of manganese, by its salutary and rapid influence, was proved superior to the iodide of potassium. The persistence of the cures obtained by manganese, in comparison with those produced by iron, is very remarkable: no cases of relapse have been observed by M. Hannon. The quantity required to be taken, in order to produce the desired result, is far from being so great as that of iron.

London Journal of Medicine, Sep. 1849, *p.* 871.

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18.—*On Scurvy at York Factory.*—By Dr. W. Smellie.—[York Factory is a settlement on Hayes' River in Hudson's bay, and it appears that an outbreak of scurvy occurs there annually among the European settlers, while at several other settlements in the neighbourhood, the circumstances of which appear similar, the disease is unknown. On a reference to the diet used at York Factory, it appears that *kitchen vegetables are not eaten at all, except at the officers' mess; and amongst the officers Dr. Smellie has not seen a single case of Scorbutus.*]

But, says Dr. Smellie, why are Fort Churchill and Severn House exempted from the disease, seeing that the diet of these establishments is equally deficient in vegetables? The theory of Dr. Garrod, which supposes the chief cause of scorbutus to be a deficiency of potash in the diet, and that a supply of this alkali in the form of its salts will be found an unfailing antiscorbutic, appears very plausible at first, when we consider that the pure citric acid is inert in scurvy; but the table of the York Factory rations shows that the great proportion of the daily food consumed is not wanting in that alkali; and it may likewise be necessary to state, that these rations are alternated one with the other during the year. As to potash of itself proving antiscorbutic, I may observe that I had a patient, a few months ago, who took scurvy while he was taking daily nine grains of hydriodate of potash for a rheumatic affection

of the joints. Neither are the rations deficient in nutritive azotised matters, the want of which is supposed by Professor Christison to be the cause of this disease.

Dr. Anderson says—"Food appropriate to man must consist of three parts—the nitrogenous, to nourish; the non-nitrogenous, to produce the extra heat required; and a third element to aid in the assimilation of these. This element is furnished by the various usually more or less acid juices contained in vegetables and fruits. In favourable circumstances man may dispense with them, and live in tolerable health, but withal obnoxious to disease: and if he be weakened, as by confinement or depression, or previous disease, or if the nourishment in the food be scanty, scurvy comes on." This is a guarded and correct statement of the case; but Dr. A. has gone too far in describing the antiscorbutic property of milk to the lactic acid developed during its digestion. Other vegetable acids found during the changes which vegetable matter undergoes, have not the same effect; why should the acid produced in badly fermented bread not be equally efficacious as an antiscorbutic? The individuals in this establishment, who use chiefly sour bread, do not on that account enjoy immunity from scurvy.

It appears then, that "in favourable circumstances, man may dispense with the use of succulent vegetables or fruits, and live in tolerable health,"—as is evident from the condition of two of these settlements on the coast; but the nature of these favourable circumstances still affords matter for inquiry. The water appears in some degree to predispose to scurvy; because, when the disease comes on early in the season, before the frost relaxes, it invariably shows itself first among those engaged in procuring timber, at the distance of some miles, where they lodge in a hut from October till April, and where the water is more of a swampy and stagnant nature than the river water used at the factory. This is the only point in which their circumstances differ, as their rations are entirely the same, and their accommodation comfortable, and in some respects more salubrious than at the factory—their dwelling in the woods being less crowded and more cleanly.

The situation of York Factory is proved to be a very insalubrious one; but the particular cause of this I am unable to determine. It is certainly a swampy district, and were it not fortunate in having eight months of the twelve of continued frost, would probably become a perfect lazar-house; but the state of the system induced by residence here, is not that commonly resulting from marsh miasmata; and it prevails as much during the frosty season, when several feet of ice must be bored to reach water, as when the swamps are open. During the first twelvemonths of a residence at this place after arrival from Europe, with some few exceptions, a person becomes stouter and more healthy in his aspect; but after that period, he gradually assumes a sallow and cachectic appearance, with a predisposition to dyspeptic disorders, and bronchitic affections. Very few Europeans can maintain tolerable health for a longer period than five years; and, indeed, I would consider three

years' continued residence to be as long as was safe for any individual of ordinary robustness. There are some persons, however, who have resided here for a great number of years consecutively, and have maintained tolerable health; some have even become corpulent; but all are sallow and cadaverous in their complexion, and time is soon traced in their features. It is very remarkable how much a year's residence in the interior improves the complexion, and apparently renews the youth; and those who have for any time resided in the interior, are apt to suffer more from a return to the sea-coast than even individuals fresh from the eastern world.

This state of the system induced by residence at York Factory, does not obtain at Fort Churchill and Severn House, the latter of which is similarly circumstanced as to diet, topography, and climate, and the former differs only in respect to its topography. With these facts, then, before us, it still seen s difficult to determine why scorbutus should prevail at York Factory, and be unknown at the other two places; and all that has been written on the causes of this disease, does not lead to a solution of the enigma. Dr. Anderson's view, that "when a man is weakened by confinement, depression, or previous disease," he becomes more predisposed to suffer from scorbutus,—"when he is denied a sufficiency of succulent vegetable matter, or any other of the three things necessary to health,"—comes nearest to an explanation; but why, then, should individuals fresh from Europe, and during their first year's residence in this region, when in other respects they appear to improve in health and robustness, be the first affected with the disease.

Monthly Journal, Sept., 1849, *p.* 1061.

19.—*Observations on Gout.*—By Dr. W. Gairdner.—[From a work by Dr. Gairdner on gout, a writer in the Medical Times extracts the following observations on the treatment of that disease. Speaking of the terminations of gout, he says,]

One of these is apoplexy. The same disposition to the deposit of earthy matter, which is so manifest in the articulations of the limbs, is equally evident in the arteries and veins. Their friable coats can no longer bear even the diminished impetus of the blood. But, though the heart is now acting with less energy, the pressure of the circulating blood is, for various reasons, oftentimes greater than before. The limbs and moving powers, it is true, have diminished in bulk, but the belly is more protuberant. Fat has accumulated around the great viscera, which, joined to the congested state of these organs themselves, and the impeded circulation through them, forces an unnatural flow towards the head and brain. The records of medicine are full of examples of apoplexy, consequent on, and arising from, gout.

Another termination of gout is menorrhagia to women at the age when they usually cease to menstruate. This is very frequently seen in a mitigated degree. As it is generally at this period of

their lives that women are attacked by gout, the discharge of blood often serves the purpose of a vicarious disease and a cure. It generally alarms them much. There are cases, however, in which they are very justly alarmed. The loss of blood goes the length of extreme and dangerous flooding. But such cases are rare, and generally occur in women of loose fibre, of indolent life, and of self-indulgent habits in matters of diet.

The same observations will apply to the case of bloody discharges from the bowels,—a far more frequent termination of gout. Hemorrhoidal bleedings are very profuse, but they give less alarm, though they are liable to be confounded with hemorrhage from the vessels of the portal system, which latter are characterized by their dark fluids, of the colour and consistence of pitch. These terminations—apoplexy, menorrhagia, hemorrhoids, and melæna—are but varieties of the same affection. The seat is different, the disorder is the same. Hæmaturia is often mentioned as a consequence of gout. It is rare, even as a symptom. I never saw it terminate the disease. Hemorrhoids, indeed, and menorrhagia, though they frequently put an end to gout, seldom do so after the manner of apoplexy and melæna, by putting an end to life, but by bringing on the next, the most common and the most alarming occurrence in gout, viz., dropsy, which now claims our notice.

The most usual forms of dropsy in gouty subjects are hydrothorax and ascites, caused by the imperfect functions of the great excretory organs, the liver and the kidneys in this, and by the disturbance of the heart in that, form of the disease. General dropsy, or anasarca, is much more rarely seen in gout. It cannot surely surprise any one, that a disease which has its principal seat for many years in the central organ of the circulation, should terminate at last in dropsy. The commencement of the dropsical affection is obscure, and the attack often evanescent. It is very common to observe a manifest fluctuation, which has disappeared the next day. A similar remark may be made regarding the water on the chest. It is not unusual to see a gouty patient oppressed with what he calls his asthma, and to receive plain indications, through the stethoscope, of the presence of liquor on the chest, and yet to find these symptoms disappear in a few hours. This is easily explained by relief given to the action of the heart.

[With reference to the treatment of gout, Dr. Gairdner, while discarding ordinary bloodlettings, recommends the removal of from three to six ounces of blood. He says that the abstracting this quantity of blood does not weaken the system, but acts as a tonic, and supplants the use of purgatives, which are often injurious. On the subject of purgatives, Dr. G. says,]

For some years past I have made little use of the neutral salts as aperients in gout. The profuse watery evacuations to which they give rise are, I think, injurious. The warm, and even the more powerful vegetable aperients, such as senna, rhubarb, aloes, jalap, and

scammony, seem to me far better adapted to the constitution of the gouty. They may be given with great advantage in the form of tincture, as also associated with the warm aromatics and tonic bitters.

[The *reviewer* recommends, as an aperient in gout, a combination of sulphur and sulphate of potash. He says,]

Ten grains of *pure* sulphur, (not the admixture of sulphur and sulphate of lime, most commonly called sulphur), an equal quantity of sulphate of potash, with, occasionally, the addition of five grains of rhubarb, form a combination which we are sure will not disappoint in these cases.

[Dr. Gairdner finds that the uric acid condition of the urine is speedily changed by the administration of the alkaline phosphates, tartrates, and citrates, while such a change takes place, not at all, or to a very slight degree, under the use of the pure alkalies and their carbonates. Neither Dr. Gairdner nor the *reviewer* concur in the common opinion, that to get the full effect of colchicum in gout, we must have griping and purging, but agree that]

Colchicum never more effectually relieves the patient than when it acts silently and peacefully, without producing any evacuations whatever, or in any way disturbing the patient's comfort and ease.

Medical Times, July 21, 1849, *p*, 59.

20.— *Use of Lemon Juice in Rheumatism.*—By Dr. G. O. Rees.—Dr. Rees directed attention, some time ago, to the beneficial effects of lemon-juice in acute rheumatism, and he has more recently put forth, in the form of a pamphlet, the details of eight cases of this disease, successfully submitted to the new treatment. He considers the lemon-juice superior to colchicum in its power of affording early relief to pain, subduing fever, and shortening the duration of the disease, and the cases recorded, so far as they go, undoubtedly warrant this statement. But, with a disease so capricious in its nature as acute rheumatism, a very extended experience is necessary to arrive at a correct estimate of the value of any new method of treatment. It so often happens that a number of severe cases, occurring consecutively, get well with unusual rapidity under all kinds of treatment, that we are led to attribute the recoveries to some accidental peculiarity in the nature of the disease itself, and not to the treatment employed.

The form of rheumatic disease in which the greatest benefit would appear to have been derived from the use of lemon-juice, is acute rheumatism; and that form of rheumatic affection involving the smaller as well as the larger joints in acute inflamation, known as rheumatic gout.

In cases of pure gout, in which the inflammation is high, it is said that great advantage has been obtained, while in sub-acute and more chronic forms of the disease, the same marked benefit has

not been experienced. Nor is it found in acute rheumatism that benefit accrues from continuing the remedy when the inflammatory symptoms have been checked, and debility remains.

In doses of half an ounce to an ounce thrice daily, the lemon-juice appears to exert a marked sedative effect on the circulation; in one case, the pulse, which was 120, and full, was after one day's treatment reduced to 75, and rendered at the same time smaller; in another case, the pulse, which was 110 when the lemon-juice was first given, was in two days reduced to 100, and in four days 74. If we can rely on results obtained in one experiment, this action is manifested also in the healthy body. A clinical clerk took one ounce of the juice three times a day for three days, and carefully noted his pulse, which was naturally full, and 75 in the minute. After five doses the pulse became much weaker and more compressible, and numbered 70 in the minute; conditions accompanied by a feeling of general depression. On the third day the pulse became as low as 66, and was very small and compressible. The urine was always acid, and also natural in quantity till the third day, when it increased somewhat; the specific gravity was then 1·017, and there was a deficiency of lithic acid.

In the cases of rheumatism related by Dr. Rees, the urine was never rendered alkaline by the use of the lemon-juice, and in one case, in which the urine was alkaline before treatment, it became acid after the juice had been employed. We think it premature to speculate on the *modus operandi* of the drug, until its utility shall have been confirmed by more extended experience, and before we are in possession of more accurate knowledge as to its physiological action, more especially in reference to its effects on the urine, and the quantity of solids contained in that secretion.

Monthly Retrospect, August, 1849, *p.* 167.

[In a discussion at the London Medical Society, several gentlemen bore testimony to the efficiency of lemon-juice in the treatment of rheumatism. Mr. Headland stated that he had found the same remedy most useful in cases of obstinate *dysmenorrhœa*. After which Dr. Golding Bird said that he]—

Hoped the time was not far distant when we should treat disease according to its essential characters, rather than in reference to its name. Now, the cases mentioned by Mr. Middleton evidenced no paradox in regard to the treatment of rheumatism by alkalies and acid; for the lemon-juice and the carbonate of soda were essentially the same in their effects upon the system. He then proceeded at some length, which we regret our space does not allow us to report fully, to illustrate the effects of a morbid matter in the blood in the production of a train of symptoms which were attributed often to another cause; or rather, the condition of the blood was not looked upon as the real and essential link in the chain of causation. Yet this fact could be evidenced by an examination of the secretions, particularly the urine. He illustrated his views with much force and ingenuity by a case of ague and a case of rheumatism. In both

these there was a *materies morbi* in the blood, which, as the disease receded, was relieved by being carried off by the kidneys, a larger and larger quantity of solids being found in the urine. This he had always found to be the case in Guy's Hospital, in which he had treated these diseases on the principle of removing the *materies morbi*. He believed that a similar condition of the blood to that which obtained in rheumatism was present in the patient affected with dysmenorrhœa mentioned by Mr. Headland. Now, in rheumatism, how were we to get rid of this morbid matter in the blood? Opium would remove it to some extent; colchicum would remove it. But it was taking a more common-sense view of the subject to employ some remedy which would eliminate the morbific matter from the circulation at once, by carrying it off by the urine. How, then, were we to effect this? Why, in acute rheumatism, by the administration of cream of tartar, citrate of potash, lemon-juice, or carbonate of soda. There was little difference between the action of these neutral salts; but lemon-juice, which was a super-citrate of potash, was more quickly absorbed into the blood, and consequently more active in effecting a cure. He had, however, in his own practice, been in the habit of employing the acetate of potash in these cases. This salt, with a mixture of sugar, water, and essence of lemon, acted with marvellous rapidity. In addition to this, given every four hours, he administered five grains of the soap pill, with opium, night and morning; for this not only relieved pain, but prevented the other remedy being carried off by the bowels. These, with the vapour-bath, constituted his treatment of rheumatism, and the result had been always successful. Soda and lemon-juice equally produced an alkaline condition of the blood, but he preferred the acetate of potash, as it was not liable to be neutralised by the presence of acid in the stomach. In respect to the treatment of ague, he never began at once with an anti-periodic. He always disgorged the liver by the employment of mild mercurials, and then some quinine. He mentioned one case, however, in which the quinine failed; he then gave the acetate of potash, and the case was cured. By these remedies we immediately altered the character of the blood, and cured the disease—a much more practical and philosophical mode of proceeding than that of treating disease according to name.

Medical Gazette, June 15, 1849, *p.* 1044.

[The Editor of the *London Journal* remarks:]

Dr. Rees has made out, quite satisfactorily, the efficacy of lemon-juice as a remedy in rheumatic diseases. It seems to be one of those remedies which act by eliminating morbid matter by the kidneys; and therefore acts beneficially very much in the same way as the acetate of potash or colchicum. In March last, in a severe case of rheumatic scarlet fever, we saw very marked relief to the pains from the following prescription, which was suggested by Dr. C. J. B. Williams. It was not specially for the rheumatism that the mixture was given; but when it was suspended, the pain became,

in a few hours, as severe as before it had been taken. The draughts were given with a considerable excess of the lemon-juice, as they seemed, when so administered, to prove very grateful and soothing. To the citrates of ammonia and soda, and to the free lemon-juice, collectively, we ought, most probably, to ascribe the relief to the rheumatism; and we would be very much inclined to suggest a series of comparative experiments with the lemon-juice alone, and with it along with soda, potash, or ammonia individually. The following is the formula used in the scarlet fever case already referred to:—℞. Sodæ sesquicarb. ʒiss; ammoniæ sesquicarb. ʒss; syr. tolutani, ℥ss; aquæ ad ℥vij. M. Sumat cochlearia duo ampla quartis horis c. succi limonis cochleari medio.

London Journal, October, 1849, *p.* 927.

Dr. Theophilus Thompson, in reference to the discussion at the meeting of the society, in which it was stated that lemon-juice and acetate of potash were identical in their mode of action in rheumatism, related the case of a gentleman who, whilst taking acetate of potash for effusion into the pericardium consequent upon pericarditis, was seized with articular rheumatism; this increased in severity so long as the medicine was continued. The urine contained a large quantity of lithates; the heart symptoms abated. Lemon-juice was substituted for the potash, and in a few hours all the rheumatic symptoms had disappeared, the urine having become copious and clear. The case illustrated the fallacy of founding a theory on a few cases.

In the discussion which followed, Dr. Thompson stated that he should trust to lemon-juice only in such cases where the constitution would not bear stronger remedies.

Medical Gazette, Oct. 12, 1849, *p.* 633.

21.—*Phosphate of Ammonia in Rheumatism.*—This remedy was fairly tried by Dr. Pepper in the spring of 1846. It was given in doses of from ten to thirty grains in acute and subacute rheumatic cases. It was not found to justify the theory of Dr. Buckler, of Baltimore, who believed it to have specific powers, by a chemical agency; but patients recovered under it, though not rapidly, so as to convince me that it might prove a useful adjuvant, and sometimes substitute for colchicum. Its alkaline composition and laxative action are sufficient to account for this. In 1847 one case at least occurred in which its use was followed by recovery, where the attack had resisted other plans of treatment. In some patients diarrhœa, and in others nausea was produced; but generally it seemed a mild remedy. The vehicle preferred was aq. cinnamon.

The curative power of colchicum was found to be far from infallible. In some, aided by depletion, its effect was immediate; others were scarcely improved while taking it even for weeks.—*American Journal of Medical Sciences.*

Medical Gazette, June 1, 1849, *p* 928.

22.—*On the Localization of Morbid Growths.*—By Dr. W. H. Walshe.—The tendency to become the seat of growths, as a class, varies greatly in the different tissues and organs. Thus, while cellular tissue is their peculiarly favorite site, fibrous texture but rarely affords them a nidus. Again, the mamma, the ovary, the uterus are frequent, the lungs and brain much less common, sufferers.

The tendency to become the seat of growths, as a class, varies in the different parts of organs. Thus, the pyloric end of the stomach suffers more frequently than the rest of the organ; the epididymis than the body of the testis.

Certain organs, and certain parts of organs, have an excess of tendency to the formation of certain special growths. Thus, the uterus, the mamma, the stomach, the liver, are peculiarly prone to cancerous, as distinguished from other forms of growth; the bones are the chosen seat of enchondroma. And, again, cancer does not form indifferently in all parts of the uterus, but tends especially to invade its neck; while fibrous tumours affect a preference for the body of the organ. The large intestine is a tolerably common seat of cancer; the small is very rarely implicated.

Growths of different kinds exhibit different degrees of compatibility, as coexistences in the same body. Some growths, as cystoma and carcinoma, are sufficiently prone to appear in the same individual; others, as fibroma and carcinoma, are rare coexistences; none are actually incompatible.

British and Foreign Medico-Chirurgical Review, July 1849, *p.* 187.

23.—ON CANCER.

By Drs W. H. Walshe and J. H. Bennett.

[We extract the following analyses of some points in the recent publications of Drs. Walshe and Bennett on cancer, from the British and Foreign Review. The following is the description given by Dr. Bennett of the microscopic appearances of a specimen of true cancer of the mamma.]

"A thin section was removed from the centre of the tumour by means of Valentin's knife, and examined with a power of 250 diameters linear. It consisted of a meshwork of fibrous tissue, forming waved bands, arranged here and there in circles, varying in size. Some of these were very large (one-fifth of a millimeter in diameter), inclosing other circles, each of which was surrounded by several filaments of the fibrous tissue. Some of these circles contained numerous nucleated corpuscles crowded together, mixed with granules; others contained only a few, or nothing but granules. Here and there were to be seen several compound granular corpuscles. On adding acetic acid to the whole structure, it became more transparent. Many of the fibrous filaments became invisible, and such as remained were studded here and there with

elongated nuclei. The walls of the corpuscles were partially dissolved and rendered very transparent, whilst their nuclei were unaffected. The cream-like fluid, which could be squeezed from the surface of the tumour, contained—1st, nucleated cells; 2d, compound granular corpuscles; 3d, numerous granules. The nucleated cells were of a round or oval form, varying in size from the 1-100th to the 1-50th, or even 1-40th of a millimeter in diameter. Some contained one nucleus; others two of an oval form, varying in the longest diameter from 1-100th to 1-75th of a millimeter. Some of these nuclei contained one nucleolus, and others two. On the addition of acetic acid, the cell-wall was rendered more transparent; the nuclei were unaffected, and appeared in consequence more distinct."

This examination presents, according to Dr. Bennett, a good example of the true cancerous structure; he selects this case, indeed, as the standard of comparison; and the cells described in the extract we have just given, he designates as *par excellence*, "cancer-cells."

Another microscopic examination is of a different species of tumour of the breast. In this case the tumour was of great size, very hard and painful, the nipple unretracted, and the axillary glands unaffected. The physical appearance of the tumour was that of an amber-coloured gelatinous mass, passing into an opaque, white, firmer fibrous mass.

"The amber-coloured gelatinous portion was composed principally of a structureless blastema, containing here and there fibres of extreme delicacy, which, with careful management of the light, might be observed to assume the form of fusiform corpuscles. In the midst of the jelly-like blastema, at different depths, were a number of compound granular masses. Some of these, when brought into focus, presented a number of granules, varying in size from the 1-500th to the 1-200th of a millimeter in diameter, which highly refracted light, and were aggregated together without being inclosed in a cell wall. When out of focus, these masses presented a brownish dark shadow. Where the amber-coloured jelly was passing into the white substance, the fusiform cells became more numerous, and were mingled with a number of oval nucleated cells of great delicacy, varying in size, and especially in length, some being caudate, others spindle-shaped. A thin section of the densest part of the white portion presented a fibrous structure, wholly composed of fusiform cells, which, on the addition of acetic acid, became more transparent, whilst the nuclei were rendered very distinct."

This tumour seemed to consist of a simple blastema, in which fibrous tissue was forming. It contained no cancer-cells, but apparently only the common elements of exudation.

Let us examine how far the observations of Dr. Bennett will enable us to determine more satisfactorily than heretofore,—1, how to recognize cancer before and after death; 2, the physiological and pathological history of cancer; 3, its appropriate and most successful treatment.

1. *Recognition of cancerous growths before and after death.*

Dr. Bennett considers that the common subjective and objective symptoms, such as lancinating pains, unequal surface, hardness, elastic feel, ulceration, affection of surrounding parts, constitutional cachexia, and return of the growth after excision, are symptoms which are not peculiar to cancer; being occasionally absent in tumours which are undoubtedly cancerous, and being present sometimes in epidermic, fibrous, or fatty growths of the most innocent nature. Accordingly he considers that an accurate microscopic examination, taken conjointly with such evidence as may be afforded by the symptoms and progress of the case, is absolutely necessary before a correct diagnosis can be given. What, then, are the microscopic characters of cancerous, as distinguished from cancroid growths?

Dr. Bennett's opinion may be given in a few words: if a tumour presents, among other elements, certain cells of definite aspect, and is at the same time situated in parts which render it impossible that these cells can be epithelial, epidermic, or cartilage-cells, that tumour must be cancerous.

We may shortly condense the account which Dr. Bennett gives of their characters. The cancer-cells are of variable shape, round, oval, caudate, heart or spindle shaped, &c.; of variable size, from the 1-100th to the 1-10th of a millimeter; nucleated, singly, doubly, trebly, or more; generally nucleolated; colourless, or with melanic deposit; increasing commonly on an endogenous plan, and thus often presenting the phenomena of parent-cells, containing two or more generations of younger cells; occasionally increasing by division of the nucleus, but never by division of the cell-wall, or by a splitting-up into segments of the mother-cell. The cell-walls are dissolved by acetic acid, the nuclei corrugated, and their margins thickened. Cancer-cells never pass into fibres, they become caudate, or throw out pointed prolongations, but the transformation proceeds no further. They are contained in great abundance in the white, milky-looking juice which can be squeezed from most cancerous tumours, and they are surrounded by a greater or less quantity of a viscous fluid. Cancer-cells cannot be distinguished from young, plastic, epithelial cells, embryonal and cartilage-cells.

In this description Dr. Bennett agrees very closely with the account of the same structure given by Lebert, Walshe, and Hannover. The chief differences are—1. That Lebert does not seem disposed to admit that cancer-cells can be confounded with any other species of cell. 2. That Walshe states that acetic acid does not dissolve the cancer-cell wall, but merely renders it transparent, and that it can be restored by using an ioduretted solution of iodide of potassium. 3. That Dr. Bennett seems inclined to doubt the exogenous mode of formation of cancer-cells, although the comparatively few cells of scirrhus are said by Dr. Walshe to increase mainly in this way.

Disregarding, however, these differences, and assuming that the

characters of the cancer-cell given above are correctly detailed, can we by means of these microscopic characters distinguish cancerous from other tumours?

1. We have already mentioned that some normal cells, as of young epithelium and cartilage, are so like cancer that Dr. Bennett says they cannot always be separated. Thus, in the eighth observation he reports a case of cancer of the cheek: a milky fluid containing cancer-cells could be pressed from the tumour; cancer-cells were found in the loculi formed by the hypertrophied, curled, elastic filaments of the dermis, and in the meshes of a fibrous matrix situated beneath the skin. In this case, Dr. Bennett says,—

"The epidermic scales offered a striking resemblance to some forms of cancer-cell. I shall afterwards point out how epidermic and epithelial cells, after being steeped in water, or a thin serous fluid, may exactly resemble, in every essential particular, cancer-cells."

And again, in the 49th observation, an enchondromatous tumour of the ischium and pubis was considered by all who saw it to be cancerous. It had partially softened; the softened pulpy matter contained cells, round, angular, or caudate, with nuclei and nucleoli. These cells looked liked cancer-cells, but differed in the fact of acetic acid rendering the cell-wall more opaque instead of more transparent.

If, however, a tumour is found at a distance from skin, mucous membrane, or cartilage, and yet contains "cancer-cells," then Dr. Bennett seems to consider their presence as absolutely diagnostic. 2. Admitting this, it seems unfortunately certain, that a tumour may be truly cancerous, that is, may be connected with a peculiar constitutional taint, may be intractable, and disposed to run through a certain series of changes, and yet no distinctive cancer-cell can be detected in its structure. Without entering into the numerous discussions on this point, we may extract Dr. Walshe's statement, which is of course of the highest authority. Dr. Walshe writes thus,—

"We have known growths which have destroyed life with the cachexia of cancerous disease, and clearly exhibited the local progress and naked-eye characteristics of encephaloid growths: which, nevertheless, were composed *of non-nucleated cells, undistinguishable from those of common exudation-matter.*"

And in an after part of the same article, the following series of propositions are given:

"1. Parent-cells, containing within them sub-cells having darker nuclei, and these in turn bright nucleoli, are strongly characteristic of cancer; but such cells are rare in, and may be altogether absent from, scirrhus; encephaloid in some phases of its growth may also be without them. 2. The shapelessly-caudate cell seems significant of cancer, but it may be absent from encephaloid, and is excessively rare in scirrhus or colloid. 3. A tumour may present to the naked eye the characters of encephaloid, be the seat of interstitial

hemorrhage, affect the communicating lymphatic glands, *run in all respects the course of cancer*, and nevertheless contain no cells, but such as are undistinguishable in the present state of knowledge from exudation-cells. 4. Nay, more, while a primary malignant tumour contains these cells alone, the lymphatic glands, secondarily affected, may contain compound nucleated cells, spherical and shapelessly-caudate. 5. The true fusiform cell is an adventitious formation when it occurs in cancer, and has no diagnostic signification."

Although we are of opinion that there is no one constant microscopic character which will serve as an infallible mark of cancer *in all cases*, we are disposed entirely to agree with Dr. Bennett in attributing the highest diagnostic value to "cancer-cells" when they are present, and are free from ambiguity. They will be present also in the vast majority of true cancerous growths; still we cannot admit that their absence negatives the idea of cancer. We agree also entirely in the judicious observations made by Dr. Bennett, relative to the use of the microscope as a means of diagnosis during life, before the tumour has been meddled with, and when doubt exists as to its real nature. M. Küss, of Strasburg, has proposed to extract a portion of a doubtful tumour with an exploring needle, and to examine microscopically the portion thus cut off. In the case of open sores, a microscopic investigation of the discharge will often clear up all doubts as to the cancerous nature of the disease, by detecting at once the characteristic cancer-cells.

Admitting that the microscopic examination of a tumour is of great value in the majority of cases, but may yet in some instances be insufficient to decide the question of its nature, are there any other circumstances which, apart from the more ordinary symptoms of cancer, may serve as diagnostic marks ?

Dr. Walshe attributes considerable importance to the fact of a cancerous tumour yielding a milky albuminous juice on pressure; he says that in ultimate analysis this is the single character least likely to deceive us. Occasionally, however, no fluid can be expressed; in Dr. Bennett's thirty-fifth observation, a cancerous tumour of the mouth gave no fluid on pressure, although numerous true cancer-cells could be obtained by scraping. And in some cases, enchondroma gives by pressure a milky juice.

Another character by which it has been attempted to distinguish cancer from all other growths, is its property of infiltrating the tissues among which it lies. It extends its meshes in all directions, and in some cases, as in the liver, produces a transformation of the adjoining tissue into carcinomatous matter. Clinically, this character, as Dr. Walshe remarks, is of no value, as cancer is not necessarily infiltrated, but is often abruptly circumscribed, or even, in very rare cases, encysted; and even if infiltrated, this cannot always be recognised during life: but nosologically, this is a characteristic of great significance. Dr. Bennett follows Dr. Walshe in attributing great importance to this peculiarity. Yet a doubt may still be raised whether this infiltrating property is limited to

cancerous growths; whether it may not belong to fibrous tumours, Dr. Walshe, in alluding to the reported cases of infiltrating fibrous tumours, intimates that infiltration with common-exudation-matter, has been mistaken for fibrous infiltration. We have, however, knowledge of one case, in which a tumour, whose fibrous nature was accurately determined, had infiltrated like cancer into the adjoining parts.

It has been asserted, also, that it is a peculiarity of cancerous growths to return after extirpation; and there is little doubt that there is a far greater liability to return, than in the case of growths of simple nature. But, Dr. Bennett's observations give increased support to the opinion which has been for some years gaining ground, that fibrous, epithelial, and even fatty and enchondromatous growths, return after excision much more frequently than is generally believed.

It appears, however, that there is no microscopic character absolutely indicative of cancer, although the occurrence of diagnostic cell-forms is extremely frequent; that a cancerous tumour cannot either be distinguished in every case by the expression of milky juice, or by possessing alone the power of infiltration.

2. *The physiological and pathological history of cancer.*

In Dr. Walshe's great work on cancer, the origin of cancer is laid down with much precision and clearness. The first step in a cancerous formation, is an exudation of a peculiar blastema, which, in our present knowledge, can be distinguished from common nutritive blastema only by the difference in the resulting organized forms. Such a blastema, as a general rule, exudes completely from the vessels, seldom is arrested in the thickness of their walls, and very rarely indeed, if ever, remains within the vessels themselves. This cancerous blastema, thus derived from the blood, necessarily implies that the blood itself is in an abnormal state, when it can furnish so unhealthy a substitute for the ordinary secretive and nutritive blastema. And this inference is corroborated by almost all the facts we can bring to bear on this point, and which all tend to the conclusion, to use Dr. Walshe's words, "that a cancerous tumour, under all circumstances, is but the local evidence of general vitiation of the system." Subsequently, the cancerous deposit may remain quiescent, the cancerous cachexia which produced it having passed away; or may advance with greater or less rapidity, according to the intensity of this cancerous diathesis. If it proceed, it is destructive or malignant, partly in proportion to the degree of this preceding constitutional disease, and partly in proportion to the infiltrating power possessed by the cancer-tumour in the particular individual. Finally, the local disease itself reacts on the constitution, and impresses a deeper and more advanced influence on the general system.

These views are identical with those held by Dr. Bennett:

"The great majority of facts with which we are acquainted," he writes, "lead to the conclusion that the filaments, cells, and fluids, which together make up the tissue I have called cancerous, origi-

nate in a coagulated exudation. This is poured out exactly in the same manner as all other forms of exudation, viz., by enlargement of the capillaries, their repletion with blood, and the transudation through their coats of the transparent liquor sanguinis, which coagulating outside the vessel constitutes an exudation, more or less solid."

The following quotation expresses his views very clearly, and proves their exact identity with those held by previous writers:

"The exudation thus poured out, which I have called cancerous exudation, must differ from the exuded matter in what is called inflammation or tubercle; but in what that difference consists we are ignorant. The characters given to the blood by Rokitanski and Engel, to distinguish what they have called the cancerous dyscrasia, are so vague and uncertain, as to have no real value. Neither is there any proof that such dyscrasia consists in an excess of albumen (Engel) or of fibrine (Keller) in the blood. The latter takes place after all exudations. But although the present state of pathology does not warrant our stating positively wherein exudations differ, we cannot avoid seeing that their occurrence in various individuals produces very different results. Thus, if an exudation be poured out in a healthy individual, it produces a series of phenomena which we call inflammatory; if in a scrofulous individual, another series of changes called scrofulous or tubercular; whilst in a third person the result may be a cancerous growth. Every kind of reasoning must lead us to the conclusion, that these different changes and effects depend not upon the vascular system, which is the mere apparatus for the production of exudation,—not upon the nervous system, which leads to the necessary arrangement of that apparatus for the purpose, and not on the texture, which is the seat of the exudation, as that varies, whilst the cancerous formation is the same,—but in the inherent composition or constitution of the exudation itself. But here pathologists pause; once traced back to the blood, they are content, and they have not sufficiently taken into consideration that the blood itself is dependent for its constitution on the results of the primary digestion in the alimentary canal, on the one hand, and the secondary digestion in the tissues on the other."

Pursuing these views still farther, Dr. Bennett remarks, that a comparison between the characteristics of the cancerous, and what may be termed the simple or inflammatory, and the tubercular exudations, may lead us to some useful hints respecting the significance of each.

Let us suppose that an individual in perfect health has from any cause (injury, irritation leading to inflammation, &c.) an exudation poured out into any part, which is plus the normal exudation required for the repair of the tissues; the further changes which occur in this vary infinitely according to the part, the degree of irritation, the rapidity or slowness with which the blastema is poured forth, &c. But one or other of what may be termed the healthy or normal changes occur; the exudation becomes the me-

dium of adhesions, or is organized into false membrane, or accumulates in the interstices of organs, assuming a greater or less resemblance to them, and impeding more or less their functions, or it breaks down more or less rapidly into evanescent cell-forms, which are discharged at once from the free surfaces, or are eliminated from the body through the medium of further changes and different emunctories. The composition of this blastema no doubt varies infinitely within the healthy limits; and these variations are evidently not dependent, for the most part at any rate, on any local condition of the part in which the blastema is exuded, but are attributable to the general condition of the organism, to the purity and excellence of its blood, which is the indication of the due performance of all the varied metamorphoses of the frame.

But if, instead of this healthy constitution, the individual be tuberculous or cancerous, then in certain organs the exudation emitted from any cause does not pass through this series of normal changes, but assumes one or other of the forms peculiar to the diathesis. Thus, in young subjects, and in persons of peculiar and defective assimilating power, the exudation is below the healthy standard. It attains imperfectly to any cell-forms, and is liable to rapid disintegration and complete loss of vitality. This is the tubercular exudation, and although it is poured out in its characteristic form only on certain organs and positions, yet we see enough to convince ourselves that a blastema poured out in other situations, as in accidental breaches of continuity, although it may not possess the tuberculous character, is yet often evidently different in some way from the reparative blastema, which would be effused in a perfectly healthy condition of the system. In an older person, in whom the assimilating functions are also deranged, although in a different manner, the blastema poured out may be entirely dissimilar to the tuberculous blastema. It may have a tendency to the production of very highly-developed and reproductive cell-forms, which grow with great energy and rapidity, and retrograde and disintegrate slowly and imperfectly. Between this diathesis and the former there is therefore almost an antagonism; they can hardly exist in the same person, and the presence of the one may be said almost necessarily to exclude the other. And here the hypothesis we have given above, for at present it is no more, agrees with the facts of the case; for it is well known that tubercle and cancer occur at different periods of life, and affect different organs, while when recent they are rarely found associated.

Dr. Bennett surmises that tubercle may be connected with some derangement in the function of the primary, and cancer with some derangement in the function of the secondary digestion; and he points out that cancer commences to occur at that epoch of life, when the excretory functions begin to lose their activity.

British and Foreign Medico-Chirurgical Review, July 1849, *p.* 189.

24.—*On the Deleterious Effects of Carbonaceous Compounds on the System.*—By Dr. C. M. Miller.—We are told that the human body is made up of a certain number of gaseous and solid bodies, and that these, when arranged in a particular manner, go to form the materials of which the healthy body is composed; why then, I would ask, should not an increase or decrease of any of these matters produce disease? This would appear self-evident, but it does not seem to have had that due weight given to it to which it is entitled.

Carburetted compounds, whether accumulated in the system in a solid form, or applied in a gaseous, are equally poisonous. I purpose, then, in the first instance, to enumerate some particulars of diseases, in which there appears to be an opposite effect produced in the system, and seeing how far these views are carried out, and whether any or no practical bearing can be derived from the investigation. First, then, we may notice the antagonism which is said to exist between phthisis and intermittent fever; and here we have *in limine*, in the first, a loss of carburetted hydrogen; in the second, an accumulation of carburetted hydrogen; and I would ask, may not the spleen be looked upon as an emunctory, aiding and assisting the liver, by receiving the superfluous quantity of carburetted blood, until the latter organ is enabled to relieve itself. I know a somewhat similar view has been mooted before; but not regarding the spleen as a decarbonizing organ, I was led to this view by noticing, that in those cases where the liver, from disease, was unable to perform its function, there the spleen has been much enlarged, (vide Mr. Langstaff's Catalogue;) that in intermittent fever it is much enlarged, and it is found largest in those animals which live on the most highly carbonized matters, as the ox and sheep.

To return: the effects of naphtha and cod-liver oil may be referred to their supplying the loss of carburetted hydrogen in phthisis, and not, I think, as Dr. Hastings says, with regard to naphtha, to its acting as a solvent of tubercle.

But let us take the other side of the question. Is it not a fact that fevers of the worst description occur in the spring and autumn of the year, and more particularly in the autumn, when the largest quantity of decaying vegetable matter is covering the earth, and giving forth an emanation of carburetted hydrogen; and is it not also a fact that typhus and other fevers arise in the confined courts, alleys, streets, and places where the vegetable accumulations are allowed to remain, and where, although the quantity must be very great, yet can we detect it by no chemical tests, except with great difficulty? Dr. Garrod, in his excellent lectures, published in *The Lancet*, says, that in 10,000 volumes of dry air, there have been found four volumes of carburetted hydrogen, but does not say at what season this analysis was made. Mr. Taylor says, that about nine per cent. may destroy life in man. If it take so little to destroy life, surely a less quantity must act as a poison to a certain extent. We have never, I believe, yet had clearly explained wha

marsh miasma is; but does there not then appear already a strong concurrence between the two causes of disease.

In fevers, again, the secretions are stopped; the skin, the kidneys, the liver, refuse to act; and what is their action? Is it not the elimination of matters almost entirely composed of carburetted compounds?

Let us for a few moments take a view of some of the effects of disease on these several organs. With regard to the skin, it almost always sympathizes with the rest. But take the kidneys, and of its diseases the worst form, ischuria,—what is the effect of a suppression of urine? Does it not produce typhoid symptoms,—showing that a powerful poison has been absorbed into the system, enervating the nervous centres? Take jaundice: what are its symptoms? Do they not all indicate the same kind of nervous depression? Again, in fevers, erysipelas, and jaundice, when the liver does begin to act, there is generally, and indeed, most frequently, thrown off, a large quantity of dark, and in many instances almost carbonaceous matter; and I have noticed that when this is the case the patient does well; but if it be retained the patient continues to suffer. It is on this principle that I consider we derive the benefit from the crises of diseases: does not nature endeavour, in these diseases, to burn up, as it were, the superabundant carbon by increased respiration and pulsations; and is not the blood highly carbonized in fevers? Simon says, "in typhus the clot is very dark, almost black-red;" and in the inflammatory forms the fibrine is superabundant. Let us take an opposite condition, as phthisis. It has been suggested, and I think with some degree of truth, that phthisis is antagonistic to ague. What do we observe here? A general loss of carburetted hydrogen in all its forms, colliquative diarrhœa and sweats, increased breathing, and all conditions that will carry off the elements of fat from the system. It is true Andral says the fibrine is on the increase in the blood; but this, I think, arises from the increased absorption, and the effort nature makes to repair the injury already done to the constitution. The advocates for the employment of cod-liver oil and naphtha are, then, practically correct.

These are suggestions, and will require much practice to carry them to the proof, although, as far as fevers are concerned, I have myself tested their value with considerable success.

And now to apply this theory to some diseases. Erysipelas occurs at the damp seasons of the year, when vegetable matter is in large quantities decaying on the earth. We have spoken of the state of the secretion of the liver.

Cholera, again, occurs at those seasons and places where there is much vegetable decay going on; at least such is the case in this country; and in this disease the liver does not perform its functions. A quantity of light matter is poured out, known under the name of rice-coloured stools. My own conviction is, and it has been verified by my experience in English cholera, that the treatment is to stimulate the liver to renewed action; and not, as recommended

in the public journals, to lock up the intestines with opium, astringents, &c. There is a poison in the system, and that poison must be got rid of.

Again, to pass from disease to the ordinary habits of life with some persons. A full meal is taken at night, perhaps an hour before retiring to rest. See the quantity of solid matter that is taken into the blood; and what are its effects? It acts as a poison; the individual has heavy, almost lethargic sleep; or is restless and dreaming, showing that the nervous system is oppressed; he has no perspiration of much amount; rises with a dry, brown tongue, and, in some instances, slight sordes round the teeth; in fact, he is in a state somewhat similar to typhus; and from it he experiences no relief until he has had a discharge which shall carry off the superfluous hydrocarbon. Again, after taking wine or spirits, the same thing occurs; and in delirium tremens the alvine discharges will frequently be nearly black.

But in the case both of eating and drinking, if the party take sufficient muscular exertion to exhaust the hydrocarbon, he may remain for some time in comparative comfort and safety.

When we take into consideration the vast quantity of carbonaceous matter which is daily taken into the stomach of even the most abstemious person, consisting of animal, vegetable, and fluid materials, and how much of this is superabundant, is it not feasible that this, if kept in the system by a suppression of the natural secretions which carry it off, should act as a poison?

Mr. Taylor, in his work "On Medical Jurisprudence," says the symptoms of poisoning by carburetted hydrogen are,—"Vertigo, cephalalgia, nausea with vomiting, confusion of intellect, loss of consciousness, general weakness and depression, partial paralysis, convulsions, and the usual phenomena of asphyxia;" and how many of these symptoms do we find in the diseases already mentioned? In a greater or less degree, almost all of them.

If, then, it be not true that carburetted compounds are the poisons which are taken into the system, there is a verisimilitude about the whole which is very striking, and which is worthy of a much deeper notice than it has hitherto attracted.

I believe that much more danger, and certainly much more disease, is attributable to the emanations from decaying vegetable, than from decaying animal matter; and in such a case it is more attributable to the carburetted compounds than any other. I state this more from experience than theory. During upwards of three years that I was connected with a public dispensary, and at which time I had nearly eight thousand cases under my immediate care, the severest were always those which occurred in situations where vegetable matters were allowed to collect.

It is interesting to examine what quantity of carbonaceous matter is passed off by the natural secretions of a person in health.

The kidneys secrete, according to Simon, a mean of 432 grains of urea in twenty-four hours. Now, on referring to the table at the end of Dr. G. Bird's work, we find that 100 grains of urea con-

tain 19.99 grains of carbon. Now if, for convenience, we take the quantity passed at 450 grains, we have 89.95, or nearly ninety grains of carbon passed off by the kidneys. The quantity of uric acid is thirteen grains, or, for convenience, we will call it ten grains, giving 3.6 grains of carbon. The colouring matter of the urine is here left out of the question, but, according to Dr. Bird, it is from forty to sixty grains, and this taken at fifty, will give carbon 29.2. There is a great uncertainty as to the quantity of bile which is secreted during the twenty-four hours, but let us take it at one ounce, or 480 grains, say 500. This gives a further supply of 342 grains of carbon.

The perspiration carries off, according to Seguin, quoted by Bird, 107 grains of organic matter, mostly nitrogenous, but still, no doubt, there is some carbonaceous matter. Taking this at thirty-five grains, or rather more than one-third, we shall have the following total of carbon passed off in a healthy state of body:—

Urine	...	...	...	119.42
Bile	...	...	...	342.
Perspiration	...	...	...	35.
Total	...	...		496.42

or, in round numbers, 500 grains. If, then, even half this quantity be retained in the blood, have we not sufficient evidence of a large quantity of poisonous matter acting on the nervous system? Our plan of treatment becomes more rational, and we are enabled to place medicine on a much firmer basis, when such considerations as these are taken into account, in reasoning on the sources of disease. I purpose, at a future period, giving some cases in illustration of this theory.

Lancet, August 4, 1849, *p.* 121.

NERVOUS SYSTEM.

25.—ON REFLEX SENSATIONS.

By Dr. R. B. Todd.

The physical change developed in the production of an objective sensation at one part, may give rise to what may be compared to a subjective sensation in another and a remote part of the body. The irritation of a calculus in the bladder will give rise to pain at the end of the penis, or to pains in the thighs. The object by which the irritation of the bladder is excited, cannot exercise any direct influence on the nerves of the penis or of the thigh: through the nerves of the bladder it excites that portion of the cord in which both the vesical nerves, and the nerves of the penis and of the thigh, are implanted; and thus the latter nerves are stimulated at their central extremities through the influence of the peripheral

stimulation: in other words, the physical changes excited in the first are reflected into the second.

Sometimes distant and apparently wholly unconnected parts may be affected in this way. Thus irritation of the ovary will cause pain under the right or left mamma; stimulation of the nipple, whether in male or female, gives rise to peculiar sensations referred to the genital organs; ice suddenly introduced into the stomach will cause intense pain in either supra-orbital nerve; acid in the stomach is apt to cause a similar pain, which may be very quickly relieved by the neutralization of the acid. Phenomena of this kind imply some closeness of connection between the nerves of the sympathizing parts in the centre, probably by means of commissural fibres connecting the respective points of implantation of the nerves with each other.

Medical Gazette, Aug. 10, 1849, *p.* 244.

26.—*On Some of the Causes of Insanity.*—By Dr. A. J. Sutherland.—[At a meeting of the Royal Medical and Chirurgical Society, Dr. Sutherland, in remarking upon the obscurity of the causes of insanity, observed,]

The disorder might take its rise primarily from the nervous centres, or the different organs of the body. The liver, the uterus, and the stomach, might affect the brain, and insanity might be the consequence. Insanity, therefore, might be said in one sense to be idiopathic: in another sense, symptomatic: but whether the one or the other, it was the seat of the intellect and the affections which was deranged; and therefore we looked with peculiar interest to the cerebrum, to throw some light upon these difficult investigations. With respect to post-mortem examinations, his experience corresponded with the facts recorded by Dr. Webster. He had never found the results of acute inflammation in the brains of lunatics, and what Mr. Solly states with regard to the cortical structure is certainly correct—viz., that there is, for the most part, a state of hyperæmia in acute cases, a state of atrophy in chronic cases; there was also a disposition to venous congestion in some brains,—to active congestion in others; but the effusion into the sub-arachnoid tissue and into the ventricles, spoken of by Dr. Webster, and so frequently found in these cases, was rather the effect than the cause of the disease: in the acute stage it was probably the result of venous congestion, and in the chronic stage it was due to atrophy of the brain; just as the spinal marrow becomes atrophied in tabes dorsalis. But he apprehended there were few who would think that what we saw after death was all that had occurred during the progress of the disease, and therefore the pathologist would be inclined to apply to the symptoms which he had seen during life, and to the analysis of the different fluids of the body, to help him in his investigations. With respect to the former, the symptoms which the intellectual faculties furnished

were those of undue activity on the one hand, and of extreme dulness on the other, while the physical symptoms were generally those of irritation; sometimes of congestion, never of acute inflammation. From the analysis of the blood of insane patients, we know that there was no excess of fibrin; while, from the analysis of the urine, we obtained sometimes a plus quantity, sometimes a minus quantity of the phosphates: thus confirming, as far as it went, the evidence which we derived from other sources. With respect to the analysis of nervous matter, L'Héritier had shown that there was a minus quantity of phosphorus in the brains of idiots, and Couerbe asserted that he had found a plus quantity of phosphorus in the brains of maniacs. But not only was it a matter of importance for us to ascertain the quality of the blood, but it was also requisite to estimate the quantity which circulated in the brain. Not only had we examples of insanity from the poison of other diseases from bad blood, but we had examples from local congestion of the brain, and from anæmia. Many patients were admitted into St. Luke's, whose disease had originated in low diet and starvation, --in whose brains might be supposed to have commenced that process of oxidation which Liebig called "eremacausis." But we should also feel inclined to examine into the condition of that subtle fluid, the nervous force, which ministered to those influences by which the mind manifested its ideas, and which, when disordered, counteracted and obscured its development. These were the heads of some of those subjects which he trusted at no distant period might throw light upon the pathology of insanity.

Medical Gazette, May 25, 1849, *p.* 913.

27.—*On the Diagnosis of the Different Kinds of Muscular Paralysis.* By Dr. Marshall Hall, F.R.S.—The kinds of muscular paralysis, of which I propose to trace the diagnosis, are two—1, the *cerebral;* 2, the *spinal.*

I designate that *cerebral* paralysis, in which, by whatever means or disease, the influence of the *brain* is *severed;* and I designate that form of paralysis *spinal,* in which, from whatever cause, the influence of the *spinal marrow* is *severed.*

It is unnecessary to state, that in the latter case the influence of the cerebrum is removed, as well as that of the spinal marrow.

Before I proceed, I must take the opportunity of briefly alluding to the *functions* observed in the muscular system, for it is these that are variously affected in paralysis.

The first function of the muscular system which I shall mention is *voluntary motion,* the effect of *volition;* the second function, or rather affection of the muscular system, is the effect of *emotion;* the third is *reflex action;* the fourth is the *tone* of the muscular system; the fifth is the *irritability* of the muscular fibre. *All* these are wonderfully involved in every muscular act or action, though *one* may be principally so, and the affection of *each* becomes to the physiologist

the source of diagnosis in cases of paralysis; for I may take this opportunity of stating, that—

In the diseases of the nervous system, the physiology is the diagnosis.

I will briefly detail the particulars of two cases, which may be aken as *types* of the rest.

The first was a case of hemiplegia, affecting the *right* side. The muscles of the right arm were paralytic, atrophied, and contracted, and rigid. We placed *both* hands in one basin, containing water with a little salt, and a foot in another, and we passed a galvanic current through the limbs so disposed, beginning with the *smallest* force which would produce an obvious movement. Invariably, in many trials, the paralytic arm was moved by the *slighter* force, and the most by a greater force, whatever the direction of the current.

The second case was that of a little girl, aged two years and a half, in whom the left arm had been suddenly affected with complete paralysis nearly two years before, the head having at the time of the seizure been drawn to one side, and the child being under the influence of dentition. In this case the paralysis of muscular power was, as I have stated, complete; the arm was atrophied, the muscles flaccid, the hand moving on the wrist, if held in the dependent position, absolutely uncontrolled by muscular influence. We applied the galvanic *test* as before, using the *mildest* force which would produce an obvious effect. Now, the *unaffected* limb was first and most affected by the galvanism!

In these two cases we have the marked forms of *cerebral* and *spinal* paralysis respectively; in the first, we observe a very marked tone (contraction); in the second, abolition of tone, (flaccidity); in the first, we have augmented, in the second, diminished, irritability of the muscular fibre. In the first, too, there was agitation of the arm on any emotion; in the second, none. I had no satisfactory mode of testing the existence or absence of reflex action.

The physiologist, and *now*, I think, the practitioner, will see the value of these phenomena to *science*, and in their translation into *diagnosis*.

In hemiplegia, the seat of motion, the source of tone, the source of irritability, are *left;* in spinal paralysis, the seat of emotion, the source of the tone of the muscles, and of the irritability of the muscular fibre, are *removed!* It must be remembered, that *in both cases* the muscular mass was *equally atrophied*. What are the obvious physiological inferences from these facts, and what are their practical advantages in a diagnostic point of view? I leave the replies to these questions to my readers, observing, that the first must be diametrically opposed to the opinions recently advocated by the late Dr. J. Reid, by Dr. Carpenter, Dr. Todd, &c.

The cases themselves, with many others, have all been witnessed by my friend, Mr. Henry Smith, of Torrington-square, and so frequently, and so carefully, that they may, I believe, be received as accurately ascertained facts, from which equally cautious inferences may be fearlessly drawn.

Are not these inferences—first, that the cerebrum is the seat of

volition; second, that a lower portion of the encephalon (the medulla oblongata?) is the seat of emotion; third, that the medulla spinalis is the seat or source of tone or tonicity in the muscular system? and fourth, of the irritability of the muscular fibre? and fifth, that the two last are *not* dependent on nutrition merely?

I may here add that the cause of cerebral paralysis is, chiefly, the injured substance of the *cerebrum;* whilst spinal paralysis may arise from the injured substance of the *spinal marrow*, or from the injured substance or function of *spinal nerves*—any disease, in effect, which severs the influence of the spinal marrow from the muscles. In this sense the paralysis arising from *lead* is spinal paralysis. It is well known that in this disease the tone of the muscles and the irritability of the muscular fibre are diminished.

I may observe that the hand and arm contracted by tone in cerebral paralysis are contracted symmetrically, and without *dis*tortion; *all* the muscles being equally affected: the flexors overcome the extensors, the arm is bent, and the hand closed firmly but regularly. In spasmo-paralysis, *certain* muscles only are excited to contraction, and that contraction is irregular, clonic, and tonic, and the arm and hand are affected with various deformities, scarcely admitting of description, but which it would be highly interesting to depict.

Lancet, Aug. 25, 1848, *p.* 200.

28.—*On the Treatment of Certain Forms of Neuralgia, Paralysis, and Rheumatism, by the Heated Iron.*—By Dr. G. E. Day, Physician to the Western Dispensary.—[In Retrospect vol. xiii, p. 42, the reader will find some very interesting remarks by Dr. Corrigan on this subject. Dr. Day's cases confirm Dr. Corrigan's statements as to the value of "firing," now called by Dr. Day "the *thermic treatment.*" Dr. Day says:]

It consists in the instantaneous and repeated application of a small iron hammer, gently heated, to the skin. The merit of introducing this form of counter-irritation is due to the late Sir Anthony Carlisle, who early in the year 1826, published a letter to Sir Gilbert Blane, "On Blisters, Rubefacients, and Escharotics, describing the employment of an instrument adapted to effect those several purposes," and in the November Number of the *Philosophical Magazine* for the same year, addressed a letter to the editor of that Journal on the same subject. This instrument was for some time used in the Westminster Hospital; but the only information I can obtain regarding the success of Sir Anthony Carlisle's treatment in that Institution is, that he failed to cure two cases of hydrophobia, in which he freely applied it along the spine. In 1829, M. Mayor, of Lausanne, published a Memoir, "Sur la cautérisation avec le marteau," in which he shows, that by an instrument of this nature, the effect either of a mustard poultice, or of a blister, or even of the moxa, may be instantly produced. Both Sir Anthony Carlisle and M. Mayor heated their instruments by immersion in boiling water.

This method has been adopted by Trousseau, Harvieux, Vanoye, Kneeland, and others. About three years ago Dr. Corrigan published (in the *Dublin Hospital Gazette*) a series of cases treated in a somewhat similar manner; he, however, makes use of the flame of a spirit-lamp for the purpose of heating his instrument, which, from the description he gives of it, seems more portable and convenient than those previously employed. The instrument I employ is shorter and more portable than that recommended by Corrigan.

Placing the end of the forefinger on its curve, I hold the metallic extremity in the flame of a spirit-lamp for about fifteen or twenty seconds. This is a readier mode of heating it than that adopted by Carlisle. By this time, the finger becomes rather uncomfortably warm, and this I regard as indicative of the instrument being sufficiently hot for my purpose. I run it lightly, alternately raising and depressing it, over the place affected. The whole operation is over in a few seconds, the patients scarcely ever complain of the application being painful, and the slight discoloration of the cuticle, which usually follows, altogether disappears in two or three days.

[Dr. Day relates several cases of facial and frontal *neuralgia*, in which the application of the heated iron was followed by the best results. One of these was the case of a man who had resided for some time in India, but had been for the last six years in England. He had been subject for eight months to severe neuralgia, caused by exposure to cold and wet. Dr. Day's report is as follows:]

His countenance is sallow, and bears the signs of acute suffering. He has comparatively little pain during the day, but about half-past six every evening, a darting agonising pain comes on, which he describes as rendering life insupportable. It extends over the whole of the right side of the face, is very severe in the right temple and proceeds, but with much less severity, towards the top of the head.

It seems neither increased nor diminished by pressure over the infra-orbital foramen, or at any other point, although it may sometimes be excited during the day, by simply touching the face, or by a gush of cold air. The mere touch of the bed-clothes would often induce it at night. From prescriptions which he showed me, it appears he has taken iron, quinine, arsenic, colchicum, and iodide of potassium; and beside these medicines he had got into the habit of taking very large doses of opium. A new remedy often gave him relief for a few days, but then seemed utterly inert.

The bowels were constipated (probably in part from the opium); the urine was loaded with urate of ammonia; the skin dry and somewhat yellow; the tongue foul; the appetite bad; and on examining the abdomen, I found considerable enlargement of the liver, unaccompanied, however, with any tenderness.

I ordered a pretty sharp emetic, to be followed next morning by four ounces of infusion of senna, with half a drachm of sulphate of potash, and a scruple of sulphate of manganese, and restricted him to a milk and farinaceous diet. He promises to diminish his dose of opium by one half.

22nd.—The tongue was now almost clean; the bowels had been very freely acted on, the evacuations being well tinged with bile. The pains had been much the same, but he felt better in himself. I saw him about three hours before the period at which the paroxysm usually came on, and applied the hammer freely over the right cheek and temple. The application did not excite the nervous pain, and gave him no annoyance in any way. I prescribed nothing but a little aperient medicine, to be taken daily.

23rd.—There was no pain whatever till between nine and ten o'clock last night; that is to say, the access was totally retarded for three hours; and it was then much slighter; more, in fact, like the occasional pains felt during the day. Applied the hammer as before.

I need not follow out all the particulars of this case, as noted down at the time. In the course of a week, the opium was altogether dispensed with.

The hammer was applied seven times in all, at intervals of two or three days; and I gave him, on the 30th, a mixture of iron and quinine, taking care only to regulate the bowels.

Dec. 14th.—He called to thank me, and to report himself as well as ever he was in his life. I saw him once afterwards, sometime in January. He came to tell me he had got a situation, and was going to leave London. There had not been the slightest relapse. I desired him to write to me if there was any return of the pain; but I have not heard from him.

The treatment of this case differs from that in the previous ones, in the fact of the hammer being applied at times when there was comparatively little pain; that is to say, before the paroxysms.

I might give several other cases, although less marked than that of Cox, in which the thermic treatment has afforded relief, after numerous other means have altogether failed.

[Dr. Day next gives some cases of *facial paralysis*. The first is that of a London physician in whom symptoms of facial paralysis came on after travelling in a railway carriage with the window open (on the 17th of January.) We are told that]

The ordinary symptoms of facial paralysis regularly set in, and increased in intensity till all power over the muscles of the right side of the face, supplied by the portio dura, was completely lost. The most distressing symptom was the impossibility of moving the right eyelid, a dimness being continually present, which was only removable for a few seconds at a time, by pressure on the globe. On the 20th, Mr. Dalrymple proposed that Dr. Watson should be consulted, as, a week or two previously to the attack, there had been severe and prolonged vomiting, intense vertigo, and other symptoms indicative of mischief within the cranium. Taking into consideration these previous symptoms (and, even at this period, there was still considerable vertigo), Dr. Watson and Mr. Dalrymple prescribed one-twentieth of a grain of bichloride of mercury to be taken with compound tincture of bark three times a day, and an

open blister behind the right ear. This treatment was steadily persisted in for about ten days, with not the slightest alteration in the state of the face, although with considerable improvement in the general health. Mr. Dalrymple, who had been reading the cases recorded in the Appendix to my treatise "On the Diseases of Advanced Life," suggested that the hammer should be applied, as directed by me. The suggestion was at once adopted, and the hammer applied twice daily. On the first evening (after only two applications) the power of closing the right eye was restored, and, in the course of a week, every trace of paralysis had disappeared.

Mr. W. E., aged 47, had an attack of paralysis of the portio dura of the right side, in March, 1843, after travelling outside a coach, and being exposed to rain the whole day. Various remedies, of which, however, he cannot give a very accurate account, were tried, but electro-magnetism was the first that proved serviceable. Under this remedy the paralysis entirely disappeared in the course of the ensuing summer; but, since that time, any exposure to cold or wet is liable to bring on involuntary twitchings of the muscles originally affected, which usually last five or six days, and then gradually disappear. To use his own expression, "he looks as if he were performing a series of very exaggerated winks."

A single application of the hammer immediately arrests these attacks. He now applies it himself, whenever he feels the peculiar stiffness of the cheek which always precedes them.

[We extract the following from a number of cases of *sciatica* and *lumbago*. Dr. Day says:]

I shall now give, as briefly as I can, a few cases of sciatica and lumbago, which are often associated affections. I have notes of a very considerable number of cases in my own practice, and I have been favoured with the particulars of other cases by Drs. Miller, Palmer, and Webster of Cambridge, and Messrs. Richardson, J. C. Langmore, Webster, Aikin, &c. &c.

I have never met with a case of sciatica or lumbago, however severe or chronic, which was not much relieved by the thermic treatment.

Charles Pope, aged 41, a carman, presented himself at the Dispensary on the 5th of April, suffering from intense lumbago and sciatica of the right side, extending to the calf. He could not take off his coat, or sit down without assistance, and stated, that even the passage of flatus or fæces from the bowels, caused much pain. Was much exposed to cold and wet about six months ago, and for the last five months he has never been free from pain for forty-eight hours together. During the last three days the pain has been extremely aggravated, entirely preventing sleep. Dr. Adey (whose valuable assistance in testing the importance of this treatment I gladly take the present opportunity of acknowledging) applied the hammer freely over the whole seat of pain. He did not feel any instantaneous relief, but, on the 7th, (two days subsequently,) told us that the pain had very much abated, directly after leaving the dispensary, and that it was now *quite gone from the loins and*

thigh, and that the only remaining trace of his ailment was a dull, aching, sensation in the calf. The hammer was applied there.

10th.—No pain worth speaking of in the calf, and none whatever in his loins and thigh since the first application. I again applied the hammer to the calf. He had returned to work the previous morning, and could walk without the slightest inconvenience.

Mary H., aged 27, applied as a morning patient at my house, on the 26th of June. She had great difficulty in walking into my room; was bent very much forward, and when she finally contrived to seat herself, informed me,—and her statement was obviously true,—that any movement of the body gave her intense pain. She stated that she was quite well till about five months ago, when, shortly after her confinement, she caught cold, and had an attack of rheumatic fever, for which she was under the care of a highly respectable surgeon in this neighbourhood. She was confined to her bed for two months. When the acute symptoms had disappeared, she had still very severe pains, especially in her shoulders, loins, hips, and knees, totally incapacitating her from following her household duties, and preventing her even from dressing herself. The gentleman referred to recommended that she should become an in-patient in one of the hospitals, and use vapour baths. She accordingly entered St. George's, where she remained nearly three weeks, without any relief. She was not allowed vapour-baths, in consequence of her debilitated condition: and she was rapidly losing flesh. It is now about ten days since she returned home in a very desponding state.

On examining her chest, I find she is in an advanced state of phthisis.

As the pains in the arms, shoulders, and between the scapulæ, were the most severe, I resolved to try to alleviate them first.

I carefully observed her while the clothes about the upper part of the chest were being removed, and saw that any motion of upper arm or shoulder gave rise to extreme agony. Even the slight motion required for needlework was unbearable.

I applied the hammer very freely, warming it twice, over the shoulders, between the scapulæ, and along the arms. I then desired her to try to raise the arms. She tried very cautiously, and obviously expecting to be, as before, checked by the pain. The arms gradually rose till she clasped her hands together above her head. The poor woman burst into tears at finding the power of her arms thus suddenly and unexpectedly restored to her. When she became a little more composed, I made her try to move them in various directions, which she found no difficulty in doing. She fastened the back of her dress without the slightest assistance, which she had not been able to do since the commencement of her illness; and left me, promising to attend in two days, when I was to apply the hammer to her loins and thighs.

I may, perhaps, be allowed to state that I was so struck with the suddenness and completeness of the relief afforded to the poor suffering woman, that I requested a lady, who happened to call at

my house at the time—an old and much respected patient of mine —to see the woman before she left. This lady has kindly permitted me to refer to her, should any of my readers suppose the above account in the slightest degree overdrawn.

June 28th.—This day I applied the hammer very freely and extensively over the loins, down both thighs, and over both knees. Great relief was afforded; she was able to walk with considerably more freedom, and with little pain. She can still move the arms without the slightest pain, but complains of their being somewhat stiff.

July 5th.—She had now a feeling of stiffness and occasional aching (especially at night), but no pain worth speaking of. I applied the hammer to the points where these sensations were most marked.

I have since seen her three or four times; there has been no return of the pain, but she is sinking fast from the pulmonary affection.

Medical Times, August 11, 1849, *p.* 110.

29.—*On the Difference between Tetanus and Hydrophobia.*—By Dr. Marshall Hall, F.R.S.—Hydrophobia, like the effects of strychnine, is *hæmatogenic.* The spinal marrow is in a state of abnormal and extreme *excitability* or erethism. But, as there is no permanent *excitant*, there is no spasm, except under the influence of an occasional one. There is perfect *inter*mission of all spasmodic affection in the intervals between the paroxysms.

Tetanus, on the contrary, is *neurogenic;* and whilst the spinal marrow is equally in a state of undue *excitability*, there is a constant source of excitation in the injured, irritated nerve. There is, therefore, constant spasm, and *re*mission only, in the intervals between the paroxysms—that is, in the absence of occasional excitants,—a paroxysmal exacerbation of spasm taking place when these are in operation.

Such is the grand pathological distinction between these diseases. I may make an additional remark: if in a case of tetanus the injured nerve was divided, the *re*mittence of spasm would become *inter*mittence. This view suggests the real value of amputation in such cases, discussed by Larrey and by Dupuytren, the former recommending, the latter dissuading from, this measure.

I may add, that hydrophobia having its source in the *blood*, its symptoms are uniform; there is always and equally laryngismus, whereas the symptoms of tetanus vary with the special nerve injured, and there is sometimes laryngismus, sometimes pharyngismus, sometimes not, as there is, in different instances, emprosthotonos, episthotonos, pleurosthotonos.

Lancet, Aug. 25, 1849, *p.* 200.

30.—*On Hemicrania.*—By Dr. A. Turenne.—1. Hemicrania implies moderate compression of a sensory nerve, and particularly of the trifacial.

2. The causes of this compression are some venous reservoirs gorged with blood, and particularly the cavernous sinuses.

3. The treatment ought to consist, 1st, in preventing the compression, which constitutes the prophylaxis of headache. It borrows its means of action from hygiene. 2nd. In palliating its effects by movements and positions of the head, &c., this constitutes the palliative treatment, and its means of action are borrowed from gymnastics. 3rd. In removing these effects, this is obtained by the co-operation of gymnastics and of stimulating, depleting, and other medicinal substances.

4. The cause of the accumulation of blood in the venous reservoirs of the base of the cranium ought by no means to be neglected.

Lancet, Aug. 18, 1849, *p.* 179.

31.—*Case of Tuberculous Tumours in the Brain and Spinal Cord.*—By — Shaw, Esq.—[The symptoms in this case, that of a woman who was a patient in the Middlesex Hospital, were at first, pains in the back and legs, followed by tingling and pricking, and then by complete paraplegia. She also complained of pain in the lumbar vertebræ.]

On the post-mortem examination no morbid appearance presented itself till the theca vertebralis was slit open. The membranes were then seen to be quite healthy; but at two points, the upper one opposite to the eighth, and the lower to the twelfth, dorsal vertebra, the spinal marrow was slightly swollen into a globular form, and felt hard; the surface, in colour, texture, and vascularity, remaining unchanged. On making a longitudinal section of the upper swelling, an oval mass of tubercular matter, three quarters of an inch in length, and about the same diameter as the spinal marrow, of the firmness of a lymphatic gland, of uniform structure, and of a pale green hue, was seen to occupy the whole interior of the organ, and was invested all round by a thin layer of medullary matter. The structure of the spinal marrow immediately adjoining the morbid growth appeared sound. The swelling a few inches lower down was found to be caused by a similar circumscribed mass of tubercular matter, contained in the interior of the cord, but it was somewhat smaller in size. In the left crus cerebelli, close to the pons varolii, and about its middle, another tubercular tumour, in colour and size not unlike the kernel of a hazel nut, was found imbedded in the substance of the crus, at the depth of about a quarter of an inch from the surface, the adjacent medullary matter being quite healthy. There was also a tumour of the same structure, and of the size of a pea, in the left hippocampus minor. Tubercles were scattered through both lungs; and in the upper lobes some seemed to be breaking down.

Medical Gazette, June 1, 1849, *p.* 950.

32.—*On Neuroma, or Tumours of Nerves.*—By Dr. R. W. Smith, F.R.C.S.I., Lecturer on Surgery at the Richmond Hospital School, Dublin.—[From a review of Dr. Smith's magnificent folio monograph on this subject, contained in the British and Foreign Medico-Chirurgical Review, we extract the following observations:]

Tumours implicated and connected with nerves have long been recognised by pathologists; and, according to one opinion, were referred to by Hippocrates and Galen under the name of ganglions. This, however, is probably incorrect; and, at all events, it was not until the publication of M. Odier's 'Manuel de Méd. Pratique,' in 1803, that they received the name, by which they are now known, of neuroma. They may be divided into idiopathic and traumatic: those which are spontaneous, and for whose appearance no external origin can be assigned; and those which follow injuries to nerves, as wounds made in amputation.

Neuromatous tumours are generally ovoidal, and have their long axis corresponding with the direction of the nerve upon which they are seated; they may often be moved from side to side without much inconvenience. Extreme suffering is occasioned when this is attempted in the longitudinal direction. They never contract adhesions to the integuments, unless from continued pressure, nor is the skin over them discoloured; and they are scarcely ever spontaneously cured. They are subject to great variety both in size and number—occasionally no larger than a grain of mustard-seed, they have been met with of the dimensions of a large melon. Frequently single, affecting one nerve only, they are at times to be counted by the hundred, and affect all parts of the nervous system alike.

The solitary neuroma is nearly always the source of acute agony to the patient, and is most often of the traumatic variety. When kept at rest, a tingling or numbing sensation may alone indicate its presence; but, influenced by changes in the state of the atmosphere, disorders of digestion, and so forth, it is liable to exacerbation of pain terrible to witness, as severe as the acutest neuralgia, and comparable only to an electric shock darting through the trunk and branches of the affected nerve. It has been noticed that in dry weather the pain sometimes ceases altogether. Idiopathic neuroma is generally met with as a single painful tumour; yet, occasionally, these growths are numerous on the same nerve, and in such instances are often free from pain. They are relatively much less frequent in the nerves of the ganglionic than in those of the spinal system. Both kinds of neuroma may be solid throughout, or may contain a cyst filled with fluid; and it has been remarked of both of them, that when the trunk of the affected nerve is compressed above the tumour, the pain occasionally ceases entirely.

There is no ground for the assertion of Bayle, Cayol, and others, that neuromatous tumours are of a scirrhous or malignant character. They never involve neighbouring parts, nor affect the lym-

phatic glands; and do not return when once removed. It is necessary, however, carefully to distinguish those cases in which malignant diseases implicate nerves, and which are not examples of neuroma at all. Of the causes of this disease we know little; mechanical irritation of a nerve by the presence of a foreign body, pressure, blows, the division or wounding of nerves by cutting instruments, and such like injuries, may be assigned as influencing the production of traumatic neuromas; but concerning the origin of the idiopathic variety we are wholly ignorant. It seems to us better to confess this at once, than to gloss over our want of knowledge by such terms as inflammation or the rheumatic diathesis. Dr. Smith has adopted as his motto a statement of Cruveilhier's, which will probably long remain true—"*L'anatomie pathologique des nerfs est encore à faire.*"

However great may be the diversity in the causes of neuromatous growths, their anatomical characters are remarkably uniform. It has been before stated that they are either solid or cystiform: it remains to consider each of these varieties, and for this purpose we transcribe entire Dr. Smith's description of them:

"The solid idiopathic neuroma originates either in the cellular structure which connects the neurilemma or external sheath to the trunk of the nerve, or within one of the smaller sheaths, by which each of the fibres, the aggregate of which constitutes the nervous trunk, is enveloped. In these two cases the disposition of the nervous fibres upon the surface of the tumour, as also the coverings of the latter, are somewhat different. In the former, when the tumour originates in the neurilemma or its connecting cellular structure, we usually find that the nervous fibres, although flattened and increased in breadth, are not separated from one another as widely as in the latter, supposing the tumour to be of the same size in each case: they are more confined to one surface of the tumour, generally its deep aspect; but when the neuroma originates in one of the central sheaths, it presses, as it grows, more equally upon the surrounding fibres which are, in such cases, found more widely dispersed over the surface of the morbid growth; Again, in the latter case, the tumour, when large, is generally invested by a greater number of coverings than in the former; its immediate capsule is the sheath of the fibre where it has originated; external to this we find a thin investment, formed by the condensation of the cellular tissue, which constitutes the general sheath to the trunk of the nerve; this sheath or external neurilemma forms the most superficial covering, with the exception, of course, of the subcutaneous cellular texture. In some cases, the coverings of the tumour may easily be separated from one another, and with very little dissection can easily be shown to be distinct membranes; but in others they are intimately united, forming a dense capsule of considerable thickness. The degree of adhesion of the tumour to its immediate investing membrane also varies considerably in different instances, the connexion being sometimes so close, that it is impossible to remove the tumour from the sac without the aid

of the scalpel; while in other instances, when the sac is opened, the tumour can without difficulty be detached by the finger from all its connexions.

"I have never seen the capsule undergo any cartilaginous or osseous transformation; it is smooth and shining externally, and its internal surface presents in general a glistening aspect, more especially when it is but loosely connected to the tumour. The oval or oblong form, which so large a proportion of neuromatous tumours acquire as they increase in size, is to be ascribed to the circumstance of their being covered by the neurilemma, which admits more readily of their growth in the direction of the trunk of the nerve. A section of a neuroma usually discloses an exceedingly dense homogeneous texture; the surface is smooth, of a grayish-white colour, and frequently the elasticity of its structure is such that the cut surface becomes convex; it seldom presents the same shade of colour as the nervous tissue; its texture is essentially fibrous, or fibro-cellular, but so exceedingly close and dense, that the unassisted eye can scarcely even detect the course or arrangement of the fibres. In the majority of cases, the tumour is solid throughout, and everywhere presents the same appearance, and an equal degree of density; indeed the uniformity of the aspect of the interior of neuromatous tumours is not one of their least remarkable features.

"It has been stated by Dupuytren that one of the characters which distinguish neuroma from the fibro-cellular encysted tumour (or painful subcutaneous tubercle of English authors) is, that in the former there is a cavity containing fluid, the latter being always solid throughout. This I believe to be by far too general an assertion, and my experience would lead me to say that it is only in exceedingly large tumours that cavities are occasionally found; as neuroma may even attain the size of an infant's head, without any cavity being formed in it. The existence of a cavity containing fluid in the smaller neuromatous tumours, I believe to be the exception. When, however, the tumour has attained a very large size, cavities occasionally form in its interior, they vary considerably in their dimensions, do not communicate with one another, and are lined by a distinct shining membrane, which, sometimes villous, is generally smooth. Some contain a serous fluid; in others the contents resemble rather synovia or albumen, or may present the characters of purulent matter; others, again, are filled with fibrine; and finally, some are empty.

"The lining membrane, generally pale, is sometimes very vascular; but I have never seen in any of the cavities of neuromatous tumours fluid or coagulated blood, lardaceous or medullary matter, or any of the substances which are found in malignant tumours; nor have I ever known the neuroma itself, no matter how long it may have existed, or to whatever size it may have attained, assume the external character or acquire the internal structure of cancerous or encephaloid growths. The trunk of the nerve, immediately above and below the tumour, is normal in appearance, and the

intermediate portion is merely altered in form, and thrown out of its usual course; it either passes as a single trunk along the posterior or lateral surface of the tumour, or else its separated fibres are dispersed over the surface of the neuroma. In a few instances I have seen some of the nervous filaments enter the superior extremity of the tumour, although I am unable to trace them through it; but I have not yet met with any example of the passage of the entire trunk of the nerve through the neuroma, as is stated to happen occasionally: 'Mox nervus cum toto trunco suo in uno latere tumorem intrat, altero autem ex eo rursum egreditur, mox complures rami et intrant et exeunt.' (Knoblauch.) When the neuroma is of a large size, it is exceedingly difficult to trace all the separated fibrillæ joining again into a trunk below the tumour, and in some instances it is quite impossible to follow them with the scalpel; but if the tumour and the portion of the nerve connected with it be left some time in nitric acid, it will become manifest that there is no interruption of continuity in the fibrillæ. It should, however, be mentioned that in the first case recorded by Aronssohn, the author states that the continuity of the nervous fibres was destroyed; perhaps if the preparation had beeen macerated in nitric acid, it would have been found that this opinion had been hastily formed."

The diagnosis of neuroma is generally sufficiently easy: "The extraordinary sensibility of the tumour, the peculiar electric character of the pain, its terrible severity, paroxysmal type, and extension along the trunk and branches of the nerve," are symptoms alone sufficiently characteristic of the disease.

With respect to the treatment, it may be observed that every measure, short of excising the disease, has been wholly unsuccessful. This may be accomplished in three ways: in the first, the tumour is cut away, together with the piece of nerve to which it is attached; in the second, the tumour is dissected out of the mass of nerve-fibres amongst which it has grown; and there still remains the severe measure of amputation, when the disease is situated in either of the extremities. The first named operation, that of removing a portion of the nerve, together with the morbid growth, is in general a safe and practicable proceeding. No fear of any evil result need be entertained; for there is ample evidence to prove that several inches of large nerves may be removed without danger; and frequently both sensation and the power of voluntary motion return after the lapse of a few months, a sensation of remarkable coldness only remaining in all the parts supplied by the affected nerve. Reich removed four inches of the ulnar nerve; a slight degree of insensibility in the little finger being the only permanent effect of the operation. A similar favourable result has followed the removal of portions of the median and tibial nerves. Malagodi cut away a portion of the sciatic nerve in a case of neuralgia, and at the end of a year the functions of the limb were perfectly restored.

Indeed the restoration of the functions of a divided nerve is a fact painfully evident in cases where the division has been performed

for the cure of neuralgia; and to the veterinary surgeons, who frequently practise neurotomy, it is often a source of great annoyance.

Neuroma succeeding to amputation.—There is good ground for the opinion that a bulbous enlargement is the normal condition of the extremities of nerves divided in amputation. In some instances this enlargement is general, commencing gradually, and ceasing as gradually; in others, it presents the form of a distinct tumour, whose magnitude is determined by that of the nerve upon which it is seated. The general condition of the nerve-filament in these cases, is that of becoming incorporated and finally lost in the substance of the growth; but sometimes the nervous termination is sudden, and in others, filaments pass through and are traceable the whole length of the tumour. Such growths are destitute of vascularity, inclosed in a distinct capsule, and, in by far the greater number of cases, productive of no uneasiness whatever.

This condition of the ends of nerves, evidently designed to protect them from receving injury by pressure and similar mechanical causes of irritation to which they are exposed, is sometimes the occasion of neuralgic suffering of the most severe character, of which no satisfactory explanation, in the present state of our knowledge, can be offered. Dr. Smith says:

"The adhesion of the end of the nerve to the cicatrix of the stump; its irritation by osseous spiculæ springing from the extremity of the sawn bone; the including of a nervous filament in a ligature; the occurrence of a conical stump; such are a few among the many circumstances which have been stated as the occasional causes of these terribly painful tumours."

British and Foreign Medico-Chirurgical Review, Oct, 1849, *p.* 396.

33.—*Case of Traumatic Tetanus cured by the Destruction of the Cicatrix by a Red-hot Iron.*—A robust youth, aged twenty-two years, was seized with trismus on the ninth day after the receipt of a wound on the temple, when it had almost healed. He experienced a painful constriction of the chest, followed by reiterated convulsions and opisthotonos. Suppression of urine, delirium, dysphagia, and unconsciousness followed.

All other means having failed to abate the severity of the disease, M. Remy, on the seventh day of the attack, determined to have recourse to the mode of treatment advised by Larrey, viz. cauterising the cicatrix in its whole extent with an iron heated to a white heat. The symptoms immediately underwent a great improvement: the convulsive movements became less frequent, and soon ceased entirely; consciousness returned, and the urinary excretion reappeared; but the muscular rigidity continued, the slightest movement or attempt at the deglutition of fluids produced a sense of suffocation; the recumbent posture had become impossible, and the patient exclaimed against a breath of air. This condition, which lasted from four to five days, disappeared under the use of digitalis in large doses. In fifteen days more convalescence was complete.

Medical Gazette, Sep. 28, 1849, *p. 555.*

ORGANS OF CIRCULATION.

34.—ON THE EFFECTS OF BLOODLETTING AND MERCURY IN PERICARDITIS.

By Dr. John Taylor, Physician to the Huddersfield Infirmary.

[In analysing the forty cases of pericarditis published by Dr. Taylor a few years ago, he divides them into two classes, first those occurring in connexion with *acute rheumatism*, the patient being previously in *good health;* and, secondly, those occurring in connexion with *renal disease*, or in persons older than those of the first class, and previously in a *bad state of health.* All those in the second class died, but very few of the first class; and hence Dr. Taylor concludes that the age and previous health of the patients, and the nature of the complicating diseases, have more influence in determining the issue of pericarditis, than any differences in the treatment. We are told that]

The remedies whose effects are examined are chiefly bloodletting and mercury.

I. *Bloodletting.*—The conclusions arrived at are the following:—

1. The duration of pericarditis increases in proportion as the time is longer between the commencement of the disease and the first bleeding.

2. The duration of the cases bled after the first four days is greater by one half than of those bled within the first four days from the invasion of the disease.

3. The influence of bleeding was more marked in the cases in which it was copiously and repeatedly, as well as early, practised, than in those in which blood was drawn less frequently and more sparingly.

4. Pericarditis is never extinguished at once by bleeding, however early, or however copiously practised.

5. In several cases the pericarditis was suspended for a limited time. The suspension in every instance was immediately consequent upon the local abstraction of blood.

6. It is probable that renal has a longer duration than rheumatic pericarditis.

7. Bloodletting must be less copious, and is more frequently inadmissible, in renal, than in rheumatic pericarditis.

8. Bloodletting probably lessens the mortality, inasmuch as it lessens the duration of pericarditis; but direct proof of the reduction of mortality is not to be obtained from these cases.

9. The abstraction of blood by venesection, cupping, or leeches, almost invariably relieved the pain at once, but not permanently. There is no reason to believe that any one form of bleeding relieved pain more effectually than another.

10. Bloodletting never lessened the frequency of the pulse, except when there were signs of the inflammation having abated.

11. The tendency to syncope in some cases of pericarditis, renders it necessary to be very careful in abstracting blood by venesection.

12. Free venesection for pericarditis does not always prevent the subsequent appearance of serious inflammation in other internal organs.

II. Mercury.

1. The cases in which mercury was given within the first four days, had an average duration less by five days than those in which it was given later.

2. The cases in which salivation was produced within the first four days had an average duration less by two days than those in which it occurred later.

3. It is difficult to determine how much of the benefit was due to the mercury, because all the patients who took mercury were likewise bled, and in almost every instance the two remedies were first employed on the same day.

4. The author is inclined to the conclusion, that the benefit was due in greater measure to the bleeding than to the mercury—partly because the duration of the disease was more abbreviated in those who simply began to take mercury than in those in whom salivation was produced within the first four days. The administration of mercury coincided with the bleeding, but the salivation did not, and the results are just what might be looked for upon the supposition that the benefit was due to the bleeding, and not to the mercury.

5. If the production of salivation had anything like the marked influence in arresting inflammation, and in promoting the removal of its products, which it is currently believed to possess, the duration of the cases of pericarditis after salivation ought to have been much less than it really was. This is proved by a detail of the cases.

(*a.*) Salivation was not followed by any speedy abatement of pericarditis in sixteen cases.

(*b.*) Salivation was followed by pericarditis in five cases.

(*c.*) Salivation was followed by an increase in the extent and intensity of the pericarditis in three cases.

(*d.*) Friction-sound ceased two days before the mouth became sore in two cases.

(*e.*) Salivation was followed by a speedy diminution of the friction-sound in two cases; it did not cease, however, for some days after.

(*f.*) The pericarditis ceased soon after salivation in two cases; in one of them, however, it had been declining for some days before.

(*g.*) Mercury was given, but no salivation was produced in seven cases.

(*h.*) No mercury was given, nor other treatment adopted in eight cases.

(*i.*) Cases are detailed exhibiting the occurrence of various internal inflammations during the time that salivation was proceeding.

The cases comprise examples of endocarditis, pleuro-pneumonia, pneumonia, pleuritis, erysipelas, and rheumatism.

A conclusion rather adverse to the antiphlogistic powers of mercury having been drawn from the facts narrated, the author next examines the evidence upon which the contrary and more prevalent opinion is based, and infers that the evidence is not satisfactory.

Medical Gazette, July 20, 1849, p. 125.

35.—*On the Accidental Admission of Air into Veins.* By Bransby B. Cooper, Esq., F.R.S.—[Respecting the proximate cause of death after the occurrence of this accident, Mr. Cooper observes:]

It appears to me that death may take place in consequence of excessive distension of the right auricle when air is forcibly injected into a vein, or when, in a surgical operation, owing to a combination of circumstances, a large quantity of air suddenly gains admission to the heart; the right auricle would then be incapable of contracting upon the air, perhaps from that fluid not furnishing the natural stimulus to the muscular fibres of the heart; immediate death would then be inevitable, as the action of the heart itself would be suddenly and completely arrested. In such a case death would be instantaneous; but when the quantity of air admitted into the vein is less, it becomes mixed with the blood, and then the circulation is only more or less impeded—clogged, as it were, with the presence of the foreign matter. As death does not here supervene with such great rapidity, time is obtained for the employment of remedial treatment; in some cases, as I have already shown, this may be sufficient to restore the patient—in others the period of dissolution is only postponed. The supposition that when air enters a vein death is caused by the poisonous influence of carbonic acid generated within the venous canal by the reaction of the oxygen of the air upon the carbon of the blood, is, I think, unsupported by any evidence that can be drawn from the phenomena symptomatic of such an accident; I believe, indeed, that where the quantity of air mixed with the blood is not sufficient entirely to prevent the contraction of the auricle, but passes into the circulation, and the blood is propelled in a frothy state through the pulmonary artery to the capillaries of the lungs, the admixture of air with the blood seems to present a physical impediment to the passage of the blood into the capillaries—the function of respiration is interrupted, and death ensues in consequence of the blood not undergoing those changes necessary to the restoration of its vital properties. It has been proved that the presence of air in a fluid materially impedes its circulation through capillary tubes; and some experiments on this subject are detailed in an interesting paper upon the "Admission of Air into Veins," read by Mr. Erichsen at the thirteenth meeting of the Bristol Association.

If, unfortunately, air should gain admission to a vein in a surgical operation, or from any other cause, the first thing to be done

is to compress the wounded vein, to place the patient in the recumbent position, dash cold water in his face, support respiration by artificial means, and to apply warmth and stimuli—such as sinapisms, &c.—to the surface of the body, hold ammonia to the nose, and give it internally mixed with wine, if the patient be capable of swallowing. If the quantity of air admitted to the heart be sufficient to maintain the distension of the right auricle, I believe death will be instantaneous. If the quantity be not so great, the heart may be still capable of propelling it through the pulmonic capillaries: the effect would then be less rapid, but still would doubtless prove fatal. If the quantity of air admitted be still less, the vital power may be sustained until the capillaries had sent the blood through the lungs to the left side of the heart, under which circumstances the patient may recover.

Medical Gazette, Aug. 17, 1849, *p.* 267.

36.—*On the Treatment of Phlebitis.*—By Bransby B. Cooper, Esq., F.R.S.—With respect to the treatment of phlebitis there is considerable difficulty; for, notwithstanding the high degree of local action by which it is characterized, the constitutional symptoms so rapidly put on the typhoid character that depletion of any kind can only be employed with the utmost caution; promoting the secretions, therefore, and at the same time allaying irritation, is all that can be safely pursued. Calomel, antimony, and opium combined, seems to be the best remedy, particularly when assisted by a mixture of liq. ammon. acetatis, tinct. of hyoscy. and camphor mixture. The affected limb should be kept elevated, to facilitate the return of blood to the heart,—poultices and fomentations should be applied over the whole limb, and punctures made through the skin wherever it has become tense from effusion. As soon as the local irritation has been subdued by these means, bark and mineral acids, nutritious food, and porter may be employed, and may be safely continued so long as the natural secretions are duly performed. M. Blandin has recommended the early application of leeches between the original point of inflammation and the first set of absorbent glands, with the view of preventing the formation of pus: I should, however, fear that the application of leeches would be dangerous both from their depressing influence, and from their increasing the local tendency to inflammation; and if there be any analogy between phlebitis and erysipelas, the danger must be rendered still greater, and no surgeon would venture to apply leeches where there existed the least tendency to the erysipelatous inflammation.

Earthy deposits are not very frequent in the veins: they occur, however, sometimes, and are called phlebolites: they have been found in the vena cava and saphena major veins: they are sometimes loose, and Mr. Langstaff met with three calculi loose in the veins of the uterus. Morbid growths are occasionally discovered in the interior of the veins: they occur most frequently, I believe, when malignant disease exists in close proximity to a vein.

Medical Gazette, Aug. 3, 1849, *p.* 179.

37.—*On Vomiting, regarded as Antagonistic to Syncope.*—By J. G. French, Esq., Surgeon to St. James' Infirmary.— Any cause capable of so lowering the force of the circulation as to determine syncope can also produce vomiting. If a strong man lose blood to such an extent as to induce syncope, he will in all probability have a seizure of vomiting. Should this occur during incipient fainting, he will not swoon; if he have completely swooned, we may calculate on reaction on the occurrence of evomition. Thus, then, from so direct a source of enfeebled circulation as the sudden abstraction of blood from a healthy man whose stomach or system contains nothing deleterious, we may confidently rely upon the production of vomiting. The phenomenon of sea-sickness, in my opinion, consists simply of a series of attacks of syncope, followed by a series of reactions. The fact that fainting is produced by a swinging motion on the generality of individuals, is undoubted, and the relief afforded to that condition by the full and active effects of retching, is also equally certain; but as the agitation of the ship continues, these phenomena continue to hold their alternate course until either the rocking of the vessel is discontinued, or the system ceases to be affected by it.

It is highly probable that the action of many of the class of emetics, and also of some poisons, owe their emetic effects simply to their influence in depressing the heart's action, and not to any direct effect upon the stomach itself. So, if any severe shock has been sustained by mechanical injury, when vomiting occurs it is to be regarded as evidence of reaction from a state of more or less syncope which has resulted from it. If we study the physiognomy of a person in a state of syncope, and also during evomition, we find that the two conditions are exactly opposite: in fainting the countenance is completely blanched; while in evomition the capillaries are intensely injected, even to those of the conjunctivæ.

Whatever may be the powerful effects of medicines in curing insidious diseases, in syncope they are peculiarly inert compared with the immediate influence of measures suggested by the physical laws. Thus the horizontal posture is by far more restorative than any of the articles of the materia medica. In examining the mechanism of energetic vomiting, we have only to regard the powerful contractions of the diaphragm and abdominal muscles, in order to understand its effect on the circulation; direct pressure on elastic tubes containing a viscid fluid being obviously the most immediate way of emptying them. Nor should it be forgotten that the blood varies in its degree of fluidity in proportion to the force of the circulation: thus rendering mechanical action the more important.

I am unwilling to leave the subject without adding a few words on its application to cholera.

In the state of collapse, with the heart scarcely acting at all, and the blood in a peculiarly viscid condition, what measures can be so restorative of the circulation as the action of vomiting? It is a fact connected with the history of the disease, that when this

symptom is most remarkable, the disease is least fatal. It is not to be alleged against the medical profession, that deficiency of zeal or inactivity is one of the causes to which the want of success in the treatment of this disease is to be ascribed, and therefore if it be established that the act of vomiting is desirable in cholera, it becomes important to weigh well how far this doctrine is to influence the remedial art. If vomiting after shock produce reaction—if it relieve the syncopal condition of sea-sickness—if it be observed quickly to restore the circulation in the subject of hemorrhage, surely these are circumstances in which the use of emetics would never for a moment be proposed. It has even been already suggested that the very action of many of the substances which produce this effect do so only by lowering the force of the circulation: thus exciting vomiting as the act of the so-called *vis medicatrix naturæ*.

The use of emetics, then, in the treatment of cholera, is not to be regarded as desirable, for this reason: all reparative processes presuppose vital power to establish them. If, therefore, there be no vomiting, it is a proof of want of vital power to resist the influence of the disease—just as in a fatal syncope there may be no emesis—and that such an action may be there even unattainable.

I would observe that whatever view be taken of the nature of the disease, and whatever cause be assigned for the state of collapse, the treatment of that state on just physiological principles demands redilution of the blood with cold water, and that the cramps and vomiting should be regarded as directly tending to restore the circulation.

Medical Gazette, Aug. 17, 1849, *p* 278.

ORGANS OF RESPIRATION.

38.—ON THE PHYSICAL DIAGNOSIS OF PHTHISIS

By Dr R. P. Cotton, Assistant Physician to the Hospital for Consumption.

[The physical signs of tubercular disease in the lungs, vary with the period of the disease, accordingly as the stage is that of crude tubercle, of softening, or of elimination. Speaking of the signs of the first stage, Dr. Cotton first mentions the supra-or infra-clavicular *depression* which is not unfrequently observed at an early period. Any such depression on one side is to be regarded, he says, with suspicion, but the sign does not invariably occur at this period of the disease. Dr. C. then observes:]

Unless the tubercular deposit be considerable, or the neighbouring part of the lung much condensed, *application of the hand* will not detect any morbid amount of vocal fremitus in the infra-clavicular

region; and as an excess on the right side is natural, its employment is nearly limited to the examination of the left, where, if the vibration be greater than on the other, phthisis may be fairly suspected.

[At this early period, careful *percussion* will generally detect a difference in the resonance of the affected part, which becomes progressively duller as the disease advances. As to the mode of performing percussion, Dr. Cotton remarks:]

The best method is to tap upon the centre of the middle finger of the left hand pressed steadily upon the intercostal spaces, parallel to the ribs, with the extremities of the united fore and middle-fingers of the right hand,—the movement being made solely by the wrist-joint, and the elbow kept perfectly motionless.

In doubtful cases it is useful to percuss whilst the patient is holding breath after a deep inspiration, during which the affected lung being less distended than the other, may exhibit more or less comparative dulness. Diminished resonance is one of the earliest and most characteristic signs of phthisis, and I have met with but two cases in which it was calculated to mislead. These were patients labouring under sub-acute bronchitis, in each of whom one infra-clavicular region was decidedly dull, as I believe, simply from an accumulation of mucus. In these instances, however, this character changed even at each visit; and such examples are so rare as scarcely to detract from the value of percussion.

[It is *auscultation*, however, which gives us the most decided evidence of disease. The following are Dr. Cotton's observations on this means of diagnosis:]

The changes in respiration caused by tubercle are various, and depend upon its extent and position, as well as on the physical state of the surrounding lung; but they may be all included under the following heads:—viz., weak, jerking, harsh, bronchial, expiration prolonged;—which will be spoken of separately.

Simple weakness, without any other alteration, is very often met with, and may be found over the whole apex, or only in spots: in the latter case, the intensity of breathing in the immediate neighbourhood is generally increased to compensate for the deficiency; and, in the former, the other lung frequently presents the respiration termed compensative or supplementary,—which is merely a highly developed natural type, to make up for loss of energy on the other side. In some cases the weakness passes almost into the absence of respiratory murmur; but I have never yet met with a case of its total suppression,—that is to say, in which a sound more or less loud could not be heard during forced breathing. Where feebleness of respiration depends upon tubercle, it is singular to observe how progressively the sound increases in descending, until, towards the middle of the lung, it becomes similar to the other side. Weakness may be a healthy peculiarity, but as such, it is found precisely alike in all parts of the chest; therefore, unless this character is determined comparatively, no dependence can be placed upon it.

When the breathing is naturally weak, slight changes are not easy of detection; but if by percussion and other general signs, suspicion be excited, it is useful to listen whilst the patient takes some deep inspirations, when a very manifest difference is often discovered. It is so difficult to make many persons understand how to breathe forcibly, that care must be taken that the same amount of effort be made whilst each side is examined; but by practice it is easy to tell from the movement of the stethoscope, whether the breathing is performed evenly.

Where, in suspicious cases, the respiration is equally feeble at both apices—the result of an equal amount of tubercle in both lungs (an unusual occurrence), I have formed a correct diagnosis from ascertaining that at the bases it was comparatively strong; recollecting the rule previously mentioned, of there being in health a somewhat greater intensity in the former than in the latter.

In jerking respiration, the inspiration, but little changed in intensity, instead of being uniformly developed, occurs in irregular puffs; whilst the expiration is scarcely affected. This is a very distinctive and common sign, and early in its appearance, often remaining for weeks or months, and generally followed by the harsh variety,—or, if the disease does not advance, gradually subsiding, or passing into the weak type.

It is not difficult to understand the cause of weak and jerking respiration: an impediment exists to the free expansion of the lung, which in the former is partially and gradually overcome, but in the latter only by intermitting efforts.

Harshness may be a quality added to either of the above, or occur as an independent variety; in the former case, no particular explanation is required, but in the latter, a frequent and important condition obtains.

Harsh respiration is known by being louder than that of health, and conveying to the ear a distinct idea of air passing over a rough surface; it is either limited in position, or diffused over the whole infra-clavicular region—the latter generally happening with the progress of tuberculization, and is at length superseded by the bronchial form. The harsh quality exists in every degree, from the simple loss of the smoothness peculiar to health, to its passage into indistinct and small crackling sounds; as it would seem the commencement of rhonchi, characteristic of phthisis, which will be explained hereafter. Its intensity is variable, sometimes amounting to a sound positively blowing, at other times scarcely above the healthy standard; when once established, it is but too apt to increase, whilst the general symptoms betray a corresponding advance in disease: by proper treatment, however, it may often be arrested and lessened, although, when once fully established, I have never known it entirely lose its characteristic quality.

Alteration in the relative length of the two respiratory murmurs is generally owing to an increase in the expiration; but sometimes the inspiration is shortened, which gives to the other an appearance of prolongation. Whether the expiration be lengthened actu-

ally or relatively, it is equally a sign of disease; but a decrease is of no importance, since it may vary in duration from the usual standard, until it is quite inaudible. Prolongation of the expiratory murmur may exist separately, or be associated with the weak and harsh varieties; it is rarely much increased in length without being changed in intensity or quality; sometimes it is louder than in health, at others harsh, or bronchial. If one or more of such changes be limited to the ordinary seat of tubercle, they become valuable evidence of phthisis, and with the concurrence of other general and physical signs, leave no doubt as to the disease.

The cause of prolonged expiration may be explained, either by the mechanical obstacle to the exit of air afforded by the tubercular matter, or the diminished contractility of the pulmonary tissue; it is probable that both may act in combination; the inspiration may be at the same time shortened, by the decreased capacity and imperfect expansion of the lung.

Bronchial respiration commences either from an almost imperceptible transition of the harsh variety, or a gradual extension of the bronchial quality of expiration into both murmurs; even in the former case the first change is often observed in the expiratory sound, and it may be a long period before the other becomes affected; but this is not an invariable rule, for occasionally the inspiration alone is bronchial, and remains so throughout.

The respiration is naturally bronchial in some parts of the chest, but elsewhere it can only become so from the conduction of sound from more or less deeply seated bronchi, by solidity of the lung. M. Fournet, a great authority in auscultation, has taken pains in explaining the distinction between the healthy and morbid kinds, but from the varying character of both, I have failed to discover the practical utility of his conclusions; when, however, the latter has been long established, it is easily recognised by its greater harshness, dryness, loudness, and tubular character. On this subject the following rules may be found useful:—

Bronchial respiration, however slight, where it does not occur naturally, is a sign of disease; if in, or near, those parts where it may do so, its value consists in the presence or otherwise of a corresponding condition on the opposite side, since the natural kind is never found on one side only, and the morbid rarely on both, or if so, not in the same degree. Whenever the respiration is bronchial over the ordinary seat of tubercle, it may be taken as presumptive evidence of phthisis: but it is far from being a constant attendant upon the first stage, as a considerable amount of pulmonary condensation is necessary to its production; when fully established it is generally permanent, until superseded by symptoms of breaking up of the lung; although by proper treatment, its progress may be often stayed, and the amount considerably lessened.

It is important to recollect that these changes in the respiration are not, *per se*, a proof of phthisis, since they indicate merely disordered function of the lung, and are common to any alteration in its physical state, impeding the free entrance and exit of air. The

lung may be thus influenced either by tubercles alone, from their number,—inflammatory consolidation, from their irritation,—idiopathic inflammation,—or mechanical obstacles in the air-passages.

Two circumstances will generally determine their dependence or not upon tubercular disease—viz. (1) position; (2) co-existence of other signs.

1. *Position.*—It is well known that tubercles, as a general rule, are first deposited in the superior lobes; in very rapid and acute cases they may be distributed equally throughout the lung, but in chronic cases, the bases are seldom implicated, and then not until the disease is in an advanced stage: in 500 out-patients I have met with but four cases where the lower parts seemed chiefly affected, and only one instance of a tubercular cavity in the inferior lobe. They appear at first equally disposed either to occupy but one side, or to occur in one to a much greater amount than the other: in 100 cases in the first stage, they were found, on the right side only, 27 times; on the left only, 51 times; and on both sides, 22 times.

Situation of Tubercle in 100 *Cases of Early Phthisis.*

Right Apex.	Left Apex.	Both Apices.	
		Equally.	Unequally.
27	51	8	14

If, therefore, the morbid respiration show a corresponding limitation, it is a valuable sign of phthisis; if, on the other hand, it occur in other situations, it is probably produced by other diseases, of which the following is a brief sketch.

The respiration is weak over the greater part of the chest whenever an obstacle exists to the free entrance of air into the pulmonary cells: it is harsh when the surface of the latter has lost its smoothness: hence, the first is met with in bronchitis and emphysema—in many nervous affections—whenever the calibre of the bronchi is changed by spasm, as in asthma, or the pressure of tumours; whilst the latter accompanies bronchitis, and emphysema (from attendant bronchitis).

Expiration may be prolonged in any region by a mechanical impediment to the exit of air, and by diminished elasticity of the lung itself; hence it is common in bronchitis from accumulated mucus, and in emphysema from permanent dilatation of the air-cells.

Jerking respiration may attend incipient pleurisy, and pleurodynia, when it is limited to the seat of disease, and is often heard in hysterical females over the entire chest.

Bronchial respiration may occur over any part of the lung, in dilatation of the bronchi, pleuritic effusion, and pulmonary consolidation.

2. *Co-existence of other signs.*—It would be unsafe to form an opinion in all cases upon position only, because an idiopathic inflammation may occupy the upper lobes, and the physical state of the

lungs be thus rendered analogous to that produced by tubercle; although such an event is comparatively rare. Other signs, therefore, should be sought for—such as the form and mobility of the chest, and some yet to be considered: percussion and application of the hand should be practised, as they will remove any uncertainty as regards nearly all affections but pneumonia, which causes dulness of sound and increase of vocal fremitus. Lastly, in all cases, and especially where there is a doubt, the patient's general history should be taken into account, when it will rarely happen that we form an incorrect diagnosis.

Medical Gazette, May 25, 1849, *p.* 903.

[Dr. Cotton proceeds to give a brief account of the various morbid sounds heard by auscultation, especially those which are chiefly connected with phthisis. He classified the sounds into (1) those depending upon morbid secretion, and (2) those depending upon altered physical state of the lung; and gives the following arrangement of them, according to the diseases in which they occur:]

Bronchitis.	Sibilant rhonchus. Sonorous „ Sub-crepitant „ Mucous „
Pneumonia.	Crepitant rhonchus. Sub-crepitant „
Pleurisy	Friction murmurs.
Phthisis.	Dry crackling rhonchus. Humid crackling „ Sub-crepitant „ Cavernulous „ Cavernous „ Crepitant „ Pulmonary crumpling sound. Friction murmur.
Pulmonary congestion.	Sub-crepitant rhonchus. Mucous „
Dilated bronchi.	Cavernulous rhonchus. Cavernous „

In this arrangement the only sounds not occurring in two divisions are the varieties of crackling rhonchi, and the pulmonary crumpling; these are consequently *direct* signs of phthisis, trustworthy under any circumstances; the rest becomes so only under certain conditions, such as position, and co-existing symptoms, and may be called *indirect*, as indicative of disease which may or may not be secondary to tubercular irritation.

The period during which tubercle seems to act merely mechanically, by impeding the proper function of the lung, varies considerably; in rapid cases inflammatory action takes place almost

simultaneously with their deposition, but in the chronic form it may not happen for months or years, when the tubercles are said to be latent. When inflammation occurs, it is generally limited to the neighbourhood of the tubercular matter, but may extend more or less over the entire lung; in the former case its presence, as indirect evidence of phthisis, is highly valuable, and when according with the general symptoms, conclusive; but in the latter some uncertainty may prevail, although the history and progress of the case will generally lead to a right diagnosis.

I shall now describe in succession the relation of the preceding sounds to the first stage of phthisis: and first, of those which act *indirectly*.

Phthisical patients are very liable to general bronchitis, known by sibilant and sonorous rhonchi in most parts of the chest, whereby the respiration is masked, and the tubercular affection liable to be overlooked, especially as percussion here loses something of its value, because it is just possible that slight dulness may depend upon accumulated mucus: such cases should be examined as the inflammation declines, when some rhonchi will be found lingering about the upper part of the lungs;—exactly the contrary to what happens in idiopathic bronchitis. Sibilant and sonorous rhonchi, unless under such circumstances, are of little importance as evidence of early phthisis, because the local bronchitis resulting from the irritation of tubercles is seldom seated in the larger bronchial tubes.

Subcrepitant rhonchus, indicative of inflammation in the smaller bronchi (capillary bronchitis), is a valuable sign of tubercles, and, although more common after softening has commenced, may occur during any stage of the disease, particularly amongst the poor, and those most exposed to changes of temperature. Capillary bronchitis may be idiopathic, as well as secondary to tubercle; but the former invariably attacks both sides pretty equally, and begins at the base of the lungs, whilst the latter is strictly local, or occupies only the diseased side: hence comes the following practical rule,—subcrepitant rhonchi, either at the apex or base of one lung only, declare the presence of tubercles in that side; but when at both bases, or throughout both lungs, they point rather to an idiopathic inflammation; in the latter case, however, the conclusion is less certain than in the other, inasmuch as if tubercles exist to a tolerably equal extent at both apices, a similar result might happen. When capillary bronchitis attacks the base of a tuberculous lung, in most cases the upper lobe is likewise affected, and the inflammation may be heard extending fro n each point, until, by meeting towards the centre, the whole lung becomes filled with subcrepitant râles.

I have never heard a mucous rhonchus in the situation of tubercles before softening has commenced; unless it be the variety called "submucous," occurring in the smaller tubes, and hardly distinguishable from the subcrepitant, which may sometimes be met with after hæmoptysis has taken place, not only in the seat of the

disease, but in distant parts of the lung,—when it is probably due to the presence of blood.

Crepitant rhonchus (fine crepitation)—the characteristic of pneumonia—is of rare occurrence with tubercular disease; simply because the local inflammation caused by tubercle is much more frequently of the smaller bronchi than the parenchyma of the lung, and when the latter is affected it would seem to be more often in a congestive than inflammatory state. In 400 out-patients examined at this hospital, I have not met with an unequivocal fine crepitation in the vicinity of tubercle—in other words, with tubercular pneumonia; but, without doubt, it occasionally exists: I can only speak practically of its rarity. Fine crepitation at the apex of either lung should always excite strong suspicion of phthisis; but since idiopathic pneumonia sometimes occupies the upper lobes, further evidence, and particularly the general history of the case, would be required to determine its real signification.

Friction murmurs are more commonly met with during the first stage than crepitant rhonchus—that is to say, local pleurisy is less rare than pneumonia: in the same number of cases I have discovered them about ten times; they were invariably of a creaking character, and very transient. It is singular that in every instance other symptoms were but indistinctly marked, which might lead to the inference that the tubercles were situated superficially, and the pleura consequently irritated, whilst the lung itself was but little implicated—circumstances naturally favourable both to the existence and detection of local pleurisy. Whenever an unequivocal friction murmur is limited to the apex of the lung, it becomes a valuable sign, and justifies the suspicion of pleurisy resulting from tubercular irritation.

The relation of cavernulous and cavernous rhonchi to phthisis will be considered hereafter, in treating of the third stage.

We now come to the varieties of crackling rhonchi—viz. dry and humid, which, unlike any of the preceding, are *direct* indications of phthisis, and never occur in any other disease. Fortunately there are none better named, more readily recognized, or more easily remembered; and, although their mechanism is unexplained, they have been so universally found to co-exist with tubercles as to leave no doubt of their relation as cause and effect. The dry rhonchus belongs to the first stage, and the humid to the second; therefore, at present, I shall only dwell upon the former. This râle is never heard at the very commencement of the disease, but it is probable that there are few or no cases which do not present it for a shorter or longer time towards the latter part of the first stage. Its general origin may be thus stated:—after many weeks, or even months, of the earlier signs—such as diminished resonance, jerking, harsh, or weak respiration, one or two dry clicks are heard during inspiration, and about the same time the breathing becomes more impaired—either less complete, or bronchial, with prolonged expiration. One single click is enough for diagnosis, and upon such evidence alone I have often founded it;

but two, three, or more are usually heard; at first they are only with inspiration, but as the case advances they may attend both murmurs, and in proportion to their abundance with the expiratory is the increase of the disease declared. I always regard dry crackling as an unfavourable sign, as it seems to indicate either the passage of tubercles from their state of latency, or a sudden increase in their number; for, when once established, it invariably remains, until superseded by the humid variety, or other moist rhonchi, and may be heard at every subsequent examination, if not in tranquil, at least in forced respiration. Its duration is variable: in some cases it is rapidly transformed, and in others remains many weeks; but I cannot remember having heard it in the same spot longer than three months.

Pulmonary crumpling,—said by its discoverer to occur in one-eighth of all cases of phthisis,—has had its very existence denied by subsequent writers. Having had unusual opportunities of examining early cases, I have particularly attended to this subject, and can, without hesitation, declare, that, whilst I am somewhat sceptical as to his first two varieties, I have no doubt of the third, although I believe he has overstated its frequency. I have often heard a rhonchus at the upper part of the chest of a dry plaintive character, much resembling the new leather friction sound, and have thought that to something of this kind were to be referred the first two varieties; but in all such instances there have always been sonorous or sibilant rhonchi in other parts of the lung, and the murmur has not only been absent at a subsequent examination, but disappeared at the time by coughing; hence, I have generally attributed it to their modification.

On the third variety, resembling the sound of crumpling tissue paper, I have met with several instances. According to my experience, it occurs only at the apices of the lungs, distinctly conveying the idea of their expansion under difficulty; is most audible posteriorly in the supra-spinous regions; is limited to inspiration, and sometimes heard only during forced breathing; it appears to accompany only the early stage, usually before other symptoms are much developed; remains but a short period (hence, perhaps, the infrequency of its detection); and is invariably succeeded either by harsh respiration, or dry crackling rhonchus. In consequence of the doubt attending it, I once called the attention of two physicians, well practised in auscultation, to three well-marked examples, when they admitted that the sound was neither a friction murmur nor rhonchus, and that no title could convey a better idea of its character than that adopted by M. Fournet. From its rarity and short duration, it ranks not amongst the most important signs of phthisis; but, when distinctly ascertained, it may be regarded as pretty conclusive evidence of the disease.

Medical Gazette, June 1, 1849, *p.* 942.

[With regard to the value of bronchophony as a sign of morbid changes, Dr. C. observes:]

When it is the result of disease, one of two pathological conditions is declared—either a portion of the lung is consolidated, and the sound of the voice thereby better conducted to the surface, or a bronchial tube is considerably dilated. Although the latter is a comparatively rare affection, it is possible to lead to erroneous diagnosis, since percussion in both cases furnishes the same result—viz. more or less dulness; but the history of the case, and the state of the patient, would in most instances declare the real nature of the disease.

Morbid bronchophony varies in intensity, and is sometimes so loud as to be positively painful to the ear, but in other respects it is not distinguishable from the natural type, and oftentimes can be called morbid only from its position. It is in the neighbourhood of the clavicles, and in the supra-spinous regions, that its importance lies in connection with phthisis; if found equally loud on the two sides, the left is probably morbid; if there be any excess on the left, it is undoubtedly so; but a greater amount on the right is unimportant, unless it be very considerable, when tubercular disease may be suspected. Bronchophony seldom appears until the first stage is somewhat advanced, and, in the great majority of instances, is accompanied with bronchial respiration; but this is not necessarily the case; for often it is distinctly marked when there is merely a slight dulness and resistance on percussion, and the respiration is weak or jerking. When once established, it will generally continue, but, under proper treatment, may sometimes be lessened; too frequently, however, it increases, and passes by insensible degrees into pectoriloquy, which, as a sign of the third stage, will be considered hereafter. On the whole, I consider bronchophony, compared with other physical signs, as of secondary importance, from the circumstance, that in health it is subject to great variation, and in disease is usually accompanied by evidence far less equivocal; but cases may sometimes occur in which it may be brought into use as a not unimportant auxiliary.

If a patient exhibiting bronchophony, instead of speaking, be directed to cough, a sound (bronchial cough), varying in intensity, and often most painfully loud, will be communicated to the ear. Similar remarks will apply to this as to the bronchial voice, or bronchophony, and, for the same reasons, its importance may be estimated at the same amount. I confess that I rarely employ this sign, simply because there are others more easily recognized, and less liable to variation; but when the case is obscure, and our diagnosis must depend rather upon the coincidence of a number of symptoms, than the unequivocal character of any one, its employment may be useful.

I shall now briefly recapitulate, by an imaginary description of the examination of a patient in the first stage of the disease.

A person presents himself with some one or more of the general symptoms of consumption, but too obscure and equivocal to determine the real natnre of his malady. Upon examining his chest, it is possible that one side may be found slightly bulged beneath

the clavicle; but more probable, either that there is no visible difference in the two sides, or that one is somewhat more depressed than the other,—in other words, that one clavicle appears to stand out more than its fellow. Either of these excites suspicion, but is not sufficiently important to justify any conclusion, and we proceed further. We watch the ordinary movements of the chest, and direct the patient to take a full inspiration, when, perhaps, the upper regions of the two sides are not equally expanded, and if the diminution correspond with the preceding sign, there is reason to suspect phthisis; but as it will generally happen that no very evident difference is discoverable, we proceed onwards in the examination. Upon the suspected part we place the hand lightly, whilst the patient speaks, when there may be an increase of vocal fremitus;—or we ascertain whether the heart's sounds are unduly conducted to that point, recollecting that the former is chiefly applicable to the left side, and the latter to the right;—or we listen for a murmur in the subclavian artery, taking care not to place too much reliance upon its discovery. We then percuss, and find more or less dulness and resistance above, upon, or beneath the clavicle, or in the supra-spinous region. It may be that we have now the first indication of the disease, most of the preceding signs having been deficient; but should ordinary percussion be somewhat doubtful in its results, we order the patient to take a deep breath, and percuss whilst he is holding it, when a difference is perhaps more readily detected.

The stethoscope now comes to our assistance to determine the value of the previous evidence: the respiration is found weak or jerking; the expiration is prolonged beyond its healthy limits; or perhaps the case is sufficiently advanced to render it harsh, or even bronchial. It is possible that we now discover the pulmonary crumping sound, but its absence is unimportant; and very likely there are a few dry crackling rhonchi. If it should happen that our patient is suffering from bronchitis of the larger tubes, we must not be too hasty in arriving at a conclusion; but if the smaller bronchi only are implicated, we shall find subcrepitant rhonchi either all over the suspected side, or chiefly seated in the upper lobe. A friction murmur may perhaps be heard at the apex; and it is just possible, even the fine crepitation of pneumonia. If further proof be desirable, from the absence of some of the preceding, we direct the patient to speak or cough whilst the stethoscope is applied, when bronchophony or bronchial cough may be present.—And thus have we arrived at the positive conclusion that our patient is in the first stage of tubercular disease.

Medical Gazette, June 8, 1849, *p.* 991.

[In the *second stage* of phthisis, we have the signs which accompanied the first stage more extensively and more unequivocally developed, and in addition to these we find others peculiar to the changed condition of the tubercular matter. Dr. Cotton gives the following description of this period of the disease:]

A patient in the second stage will generally have the infra-clavicular region of one side visibly flattened, and less capable of expansion during ordinary and forced breathing, than the other. If the hand be applied to that part, both vocal and tussive fremitus will be found increased: and, on percussion, it will exhibit more distinctly than formerly, both dulness and resistance. The respiration will probably possess some of the morbid characters already described,—most frequently it is harsh or bronchial. The sounds of the heart are now often heard unnaturally on the diseased side; bronchophony and bronchial cough are much more developed; and the pulmonary crumpling, if it has existed, ceases to be heard.

Fortunately, there is nothing in the whole practice of auscultation more easy than to detect the very beginning of the softening process; the dry crackling rhonchus has simply to become humid, and the evidence is complete: a number of bubbling subcrepitant, or large mucous râles, have but to occupy the diseased part, and the transformation becomes more than probable.

A knowledge of the humid crackling rhonchus is therefore of the greatest importance, but is easily acquired, as it resembles the dry one, in all other respects than its moistness, having the same characteristic click. They are found to pass gradually into each other, and every intervening state, from the perfect dryness of the one, to the well-marked humidity of the other, is constantly heard; so that it is sometimes difficult to determine to which the sound should be referred. It is impossible, however, to doubt its tubercular origin; and any deviation from positive dryness, justifies the conclusion that transformation has begun.

On the first appearance of the liquid rhonchi, they seldom exceed the others in frequency, but with the advance of the case, they become much more numerous, and, at the same time, more frequent with expiration,—more distinctly liquid and bubbling,—until at length they acquire a somewhat metallic quality, and pass into others distinctive of the last stage; these changes being as gradual as in the former case. Nothing is more variable than the period of their duration, but it may be said to be decidedly shorter than that of the dry variety. The mode of their production is not thoroughly understood, but for all practical purposes this is not important: it is sufficient to know that a humid crackling rhonchus, under all circumstances, tells of softening tubercle.

Humid crackling—the large and moist subcrepitant—and the larger kind of mucous rhonchi, therefore, are signs announcing the second stage, but they differ in their value. The first is positive and unequivocal, never occurring under any other circumstance; the second and third are of less importance, since they require, like other indirect signs, considerations similar to those spoken of in a previous lecture—viz. their position and the co-existence of other signs, to determine their signification. Either of them may occur alone, or be so intermixed with each other, as to make their distinction impossible. With regard to their comparative frequency, the humid crackling by itself is the most common; next to this, is

its combination with the sub-crepitant; but the latter is often found alone, or in conjunction with the mucous;—great variety in this respect constantly takes place.

There is a variety of mucous rhonchus with a clear metallic note, very liquid and bubbling, which, although difficult to describe, is so peculiar as to render its recognition, after having once been heard, a very easy matter. I have reason to believe that the humid crackling rhonchus generally passes into it, and that the others sometimes do the same: it pronounces with great certainty that the second stage is much advanced, and not far distant from the third. When once established, I have invariably found it permanent, until, by gradually increasing, it has become lost in rhonchi characterizing the last stage, or, if the secretion has diminished, it has been succeeded by cavernous respiration. I have long regarded this sound as the sure forerunner of tuberculous excavations, and have too often been able to verify the accuracy of the conclusion.

The part where softening first shows itself, will generally be found to be that in which there were the earliest signs of crude tubercle; and if the lung be now examined, those parts where there was formerly no appreciable deviation from the healthy conditions, will exhibit more or less evidence of incipient tuberculosis. It appears that there is at this time a tendency to the rapid deposition of tubercle,—the effect of some further reduction in the resisting powers of the patient, under which, those tubercles which are the oldest undergo transformations.

Medical Gazette, June 15, 1849, *p* 1038.

[There is no positive line of separation between the second and third stages of phthisis, the one passing imperceptibly into the other; but the *third stage* is said to have commenced, when there is proof of the smallest excavation. The majority of the signs of the third stage consist merely in an increase of those met with in the second: the peculiar ones,—those which indicate the existence of cavities,—are thus described by Dr. Cotton:]

Where the cavities are small. - I have already said that the humid crackling, mucous, and subcrepitant rhonchi pass into one nearly allied to the mucous, but essentially metallic in its quality: when this is fully established the commencement of very minute cavities may be safely diagnosed. If a patient in this condition be examined from time to time, the rhonchus will be found more and more characteristic, until, by increasing in size and humidity, it at length becomes decidedly cavernulous.

The true cavernulous rhonchus is a clear, bubbling, and metallic sound, taking place both with inspiration and expiration, but especially the former; varying in loudness and frequency according to the amount of secretion; and giving to the ear the impression of being produced in a small and hollow space: it is easily recognized, and declares unequivocally the establishment of the final stage. The respiration at this time is usually more or less bronchial; some-

times merely harsh; but often distinguished with difficulty, from being masked by the rhonchi. I have observed that the heart's sounds, especially the second, now become more than ever distinct on the diseased side, which although not amongst the positive signs of excavations, may be sometimes recollected with advantage. Bronchophony is now more positive than formerly, and oftentimes so loud, as to be painful to the auscultator,—the result of increased conducting power of the lung, with emaciation of the thoracic walls.

Where the cavities are large.—When vomicæ are of some size, there arise a new set of signs, not yet mentioned, and with which it is highly necessary, but happily equally easy, to be familiar: they consist of still further modifications in the percussion sound, the respiration, the rhonchi, and vocal resonance,—all of which I shall explain separately.

Every variety of sound, from absolute dulness to what is termed amphoric resonance, may attend percussion over a cavity, according to its size, position, and the state of the surrounding parts: the force employed also makes a considerable difference, since, if gentle, it will proceed from the superficial, and, if hard, from the deeper parts. Positive dulness of sound, with perfect inelasticity of the thoracic wall, is by far the most common. Not unfrequently, however, the sound is so little altered from that of health, owing probably to the morbid clearness of the cavity balancing the dulness of the neighbouring induration, that it is not very easy, without other evidence, even to ascertain which side is diseased. Amphoric resonance is heard only over large cavities seated close to the surface, or separated from it by indurated lung, and in the latter case requires rather strong percussion for its production: it is very characteristic, and exactly imitated by fillipping the cheek when the mouth is inflated. When the percussion sound is more or less amphoric, it frequently happens that by directing the patient to keep his mouth open whilst the chest is struck somewhat smartly, a new character is developed, so peculiar, as to decide at once the history of the case,—I allude to the cracked-pot sound, first described by Laennec under the name of "bruit de pot fêlé." It is impossible by description to do justice to its peculiarity; but this is well expressed by its own designation, as it exactly resembles the noise produced by striking an empty and cracked pipkin. It seems necessary to its development that the cavity should be large, tolerably dry, and communicating freely with the bronchi; and that the mouth should be widely open. In no case have I heard it clearly marked in the absence of the last condition. Such a combination, however, does not invariably produce the cracked-pot sound, for reasons, perhaps, difficult of explanation; and if I were to hazard an opinion as to its frequency, I should say, that it may be met with in about one-third of such cases. On the whole, percussion is a capricious guide to vomicæ;—more frequently affording no evidence of their presence; sometimes calculating to mislead

even as to the nature of the disease, but occasionally announcing them in the most unequivocal manner.

By the respiration alone, cavities of tolerable size may generally be detected, from the air in passing through them giving rise to a sound which is properly termed cavernous, easily distinguished by its hollow and metallic character, and at once telling of its production in an empty space.

Cavernous respiration often has its origin in a gradual increase of the bronchial type—the one passing into the other—but it may succeed any other morbid variety, or begin in spots, where, during the second stage, scarcely any respiration could be heard. Intense bronchial respiration is very closely allied to cavernous, and from a confusion of the two by inexperienced auscultators, I have seen patients unnecessarily alarmed, and most undeserved discredit thrown upon the stethoscope. They may be distinguished by the bronchial giving rise to the idea of its originating in a dry tube, whilst the cavernous seems to be produced in a hollow space, and is distinctly metallic; but it must be confessed that cases now and then occur, in which the most experienced have some difficulty in deciding between them.

Where the cavity is of very great extent, and tolerably free from secretion, the respiration undergoes another change, and is said to be amphoric,—a quality easily recognised by its resemblance to the sound caused by blowing into an empty bottle.

Amphoric respiration is generally, but not invariably, attended by amphoric resonance on percussion, and, like the latter, requires for its development, that the cavity should be large, and situated near the surface. There is in reality no line of separation between cavernous and amphoric breathing; the one merges into the other.

The only condition at all likely to be mistaken for vomicæ, from the evidence of cavernous respiration, is extreme dilatation of the bronchial tubes—a very rare disease, more common in the centre than at the apex of the lung, and in its attendant symptoms so different from the last stage of phthisis, as scarcely to justify the one being ever mistaken for the other. Cavernous respiration is dry or humid, according to the absence or otherwise of secretion in the cavity, and in the latter case is associated with, and even frequently masked by, a very characteristic rhonchus termed cavernous, or from another of its properties, gurgling.

Cavernous rhonchus has many varieties, according to the amount and consistence of the secretion, and the size of the cavity; sometimes it resembles the bursting of large bubbles, at others, the agitation of some thick and viscid substance in contact with air, but in all instances it conveys the idea of metallic hollowness, and can scarcely fail, even to the most inexperienced, to tell at once of the seat of its production. It varies in tone, and is sometimes heard by the patients themselves, who are thus enabled to point out the exact situation of the cavity; and not unfrequently it is loud enough to be distinguished at some distance from the patient's chest. Persons with cavities will sometimes describe a sensation like the tick-

ing of a clock, and if the stethoscope be applied, an indistinct impulse is communicated to the ear, apparently produced by the agitation of fluid by the heart's action.

When amphoric resonance, or the cracked-pot percussion-sound, or amphoric respiration, announce a very large cavity, gurgling rhonchus will seldom be heard, except perhaps at the very bottom, but a new sound may be developed by an effort of the patient, such as coughing, speaking, or breathing loudly, and sometimes without it—termed metallic tinkling.

It would be useless to enter into the many speculations as to the cause of this phenomenon; it will suffice to state that the sound exactly resembles the falling of a sharp metallic substance into a glass or metal jar;—sometimes reverberating and giving rise to what has been called metallic echo; and that it declares the existence of a hollow space filled with air, and containing a fluid. I regard it as a very rare attendant upon tuberculous cavities, never having met with it in the numerous cases I have examined; in hydropneumothorax it is very common, and the anatomical analogy between the two diseases would lead to the supposition of the former occasionally giving rise to it; I can only affirm that it seldom does so.

The last sign to be considered is pectoriloquy, which is a yet further modification of vocal resonance, and bears the same relation to bronchophony as cavernous respiration does to bronchial, and cavernous rhonchus to the crackling varieties. Pectoriloquy signifies the cavernous voice, and if, instead of speaking, the patient cough, the tussive resonance will be similarly modified (cavernous cough). In pectoriloquy the sound is more circumscribed than in bronchophony, the words are more articulate, and seem produced within the stethoscope, and close to the ear. Very often, although there is abundant proof of a cavity, there is but little pectoriloquy, or it is hardly separable from bronchophony, which may arise from a want of certain circumstances indispensable to the production of the former—viz. a moderate size, emptiness, and closeness to the surface, of the vomica. The loudness of the voice has but little influence in its production; but if the patient be directed to whisper, it becomes more distinctive (whispering pectoriloquy).

Of all the signs of pulmonary excavations, I believe pectoriloquy is the least to be depended upon; because, in the first place, it is not very often met with in a perfect form; and, in the next, many healthy persons, with thin chests and peculiar intonation of voice, have what may really be termed a natural pectoriloquy. I have especially observed this amongst French persons, and attributed it to the peculiar tone of their language; and it was in the French hospitals that I first began to distrust pectoriloquy. I confess that I am now seldom in the habit of employing it in diagnosis, both for these reasons and because other signs are generally sufficient, although I admit, that when well marked, it is very characteristic; whenever I desire its assistance, I invariably listen for "whispering pectoriloquy."

The tussive resonance in cavities (cavernous cough) is of the same value as the vocal, and for the same reasons, but when well developed is perhaps even more characteristic. It is a ringing and metallic sound apparently produced close to the ear, often of sufficient intensity to cause a very painful sensation to the auscultator, and requires for its production the same conditions as pectoriloquy.

When the cavity is of very large size, and under circumstances favourable to the production both of the amphoric sound on percussion and amphoric respiration, the resonance of the voice and cough is attended with still further modification. When the patient speaks, the voice (amphoric voice) instead of being close to the ear and articulate, as in pectoriloquy, has more the character of bronchophony, but is hollow and metallic, and resembles the sound produced by speaking into an empty vessel Precisely the same quality of sound attends coughing (amphoric cough) and the two will generally be found together. On the whole I believe the amphoric voice and cough are of more value than the cavernous, being more peculiar—impossible of imitation in any healthy condition—and, under favourable circumstances, more constant in occurrence. There are but two pathological states which can produce these amphoric sounds, viz. large vomicæ and pneumothorax; but with the aid of other signs, and the history of the case, one can rarely be mistaken for the other; for whilst the former is nearly always at the upper part of the lung, and of comparatively limited extent, the latter exhibits itself over the greater part, if not the whole, of one side; the sudden invasion and severity of the attack would also suggest the idea of pneumo-thorax, by contrasting strongly with the chronic course of a large vomica.

Medical Gazette, June 22, 1849, *p.* 1089.

39.—*On Hæmoptysis, as a Symptom of Phthisis.*—By Dr. W. H. Walshe.—The diagnostic value of hæmoptysis, as a symptom of tubercle, is estimated very highly by Dr. Walshe. It appears that, with the exception of cancer of the lung, aortic aneurism, and disease of the heart, there are very few cases in which a material hemorrhage takes place from the lung independently of tubercles. Neither those cases of apparently simple chronic bronchitis, nor those of irregular menstruation, in which blood is voided from the lungs to any extent, appear to be exceptions to this proposition, although the tubercles may remain for a considerable period latent after the occurrence of the hæmoptysis. We give the following most important conclusion in the author's own words:—

"The tendency of my experience, then, is clearly to show the vast frequency with which hæmoptysis is in some manner or other an attendant on tuberculous disease. The fact, that individuals are occasionally met with who, after having had more or less hæmoptysis, live on to a good age without exhibiting phthisical symptoms, does not invalidate this result: it simply shows that

tuberculization tending to hæmoptysis may, as well as that not so tending, undergo spontaneous suspension; and the consideration of the point becomes rather a question of prognosis than anything else.

Monthly Journal, Oct., 1849, *p.* 1160.

40.—*On Intercostal Neuralgia as a Sign of Phthisis.*—By M. Beau.—Intercostal neuritis is an affection which habitually coincides with inflammation of the pleura, whether simple or combined with pulmonary inflammation. The explanation of this coincidence is found in the anatomical relations of the pleura and intercostal nerves which are in immediate contact at the posterior part of the thorax. According to the author, the *stitch* in the side, which attends affections of the pleuræ, and most severe pulmonary affections in which the pleuræ are involved, is owing to inflammation of the intercostal nerves, which are found enlarged to two or three times their normal size, adherent to the pleura or to the cellular tissue in which they lie, and, in acute cases, much injected. The degree to which the nerves are affected bears an exact relation to the amount of affection of the pleura, and is therefore evidently secondary to it.

When the lung is attacked by tubercles, it almost invariably happens that adhesions of the pleuræ take place in their neighbourhood. The adhesions, like the tubercles, are at first confined to the summit of the lung, and are attended by the same change in the intercostal nerves as in the case of acute affections, only that the lesion is chronic in its development, and not generally characterised by increased vascularity.

Thus the author explains the dull pains which occur at the summit of the thorax in phthisical persons. Sometimes these pains are sufficiently distinctly marked, and even neuralgic in character; they radiate into the neck and supra spinous fossa, or even along the arm, as in angina pectoris; this divergence being due to the inosculations between the intercostal nerves and those of the cervical and brachial plexuses. But it is not always so, nor can these pains be said to be an habitual symptom of phthisis. Nevertheless the lesion of the intercostal nerves is almost always found after death from this disease.

In seeking to explain this contradiction, M. Beau has discovered a symptom, to which he attaches considerable importance in the diagnosis of doubtful cases of phthisis. Even where no pain is manifested under ordinary circumstances, he finds that this symptom may be almost constantly educed by moderate pressure with the finger on the anterior or sternal extremity of the intercostal spaces. In early cases it is limited to the upper part of the chest, and is, generally speaking, more severe in the first space than in the second, in the second than in the third, &c. It varies in intensity, but sometimes is so considerable as to cause an involuntary shrinking on the part of the patient. It is most severe, and affects

the greatest number of spaces, on the side in which the tuberculization has advanced farthest.

This symptom is almost constant in phthisis pulmonalis. Among fifteen cases under M. Beau's observation at the time of writing, it was not absent in more than one; and only one of these cases presented the symptom of *spontaneous* pain. The author, therefore, thinks it will prove a useful diagnostic mark in cases where the physical signs of phthisis are masked by bronchitis. The predominance of the pain in the anterior part of the spaces, where the alteration of the nerves is least considerable, is ascribed by M. Beau to the circumstance that, when a nerve is diseased in any part of its course, the morbid sensibility is always referred to its peripheric extremity.

Another form of the intercostal neuralgia of phthisical patients is the pain between the shoulders which has been so frequently described as characteristic of phthisis. In these cases the author has always found that the pain may be traced along the intercostal spaces to the anterior part of the chest. M. Beau had previously endeavoured to demonstrate the connection between intercostal neuralgia and dyspepsia: he now considers both these symptoms as related to phthisis, of which disease dyspepsia, as is well known, is a frequent accompaniment.—*L'Union Médicale, July* 21, 1849; *and Archives Générales de Médecine, Feb.* 7, 1847.

Monthly Retrospect, Oct., 1849, *p.* 199.

41.—*On the supposed Hereditary Nature of Phthisis.*—By Dr. W. H. Walshe.—[The following statements, which are opposed to the prevailing opinions on this subject, are given by Dr. Walshe as the result of a careful examination of a large number of cases at the Hospital for consumption. Dr. W. says:]

From a comparative analysis of the family histories of 446 persons, 162 of them phthisical, 284 non-phthisical, the final conclusion flows:—*that phthisis in the adult hospital-population of this country is, to a slight amount only, a disease demonstrably derived from parents.* It is possible (nay, indeed, *probable*, for, in adults having a parental taint, the outbreak of the disease occurred a mean period of two years and nine months earlier than in those free from such taint.) that, were investigation extended to infancy, childhood, and youth, the ratio of cases of parental taint among the phthisical, would be proportionally greater than it proves where inquiry is limited to adults. But on the other hand, there is no single valid reason for *supposing* (prior to actual experience) that the increase in that ratio would be of more than trifling amount. Again, whether the law differs in the adult portion of the middle and upper classes of society, from that holding in the humbler classes (those supplying hospitals), can only be positively determined by an analysis of family histories collected among the former classes; meanwhile it appears justifiable to doubt the reality of any such difference.

Monthly Journal, Oct. 1849, *p.* 1159.

42.—*On the Nature of Scrofula and Tubercle.*—By Dr. W. Addison, F.R.S.—[Dr. Addison shows in the first place, that the earliest stage of every texture is characterised by corpuscular or cell-growth.]

The cells possess, at first, very slight coherency; at a later period, in a higher development, they form a coherent tissue, which is illustrated in plants by the chlorophyll-cells of the leaf, and in animals by the parenchyma of the liver, and many other structures. A still later development is that of fibres and tubes both in plants and animals; and last of all come the most highly elaborated textures, peculiar to the latter,—cartilage (?) and bone.

The law of metamorphosis of organised structures may be expressed as follows—The lowest type is found in non-coherent cells; the next in cellular tissues; the next in fibres and cells conjoined; and the highest in dense structures, such as cartilage and bone. The metamorphosis is *regular* when all these structures are developed according to the normal type; it is *irregular* when all the elements are nominally present, but in altered amount or grouping; and it is *retrograde* when the higher phase is in any instance exchanged for the lower.

In the permanent tissues of the adult animal, all these different types may be found. In the more dense and coherent structures, whose function is chiefly mechanical, the fibrous and the higher types prevail; in the secreting structures, on the contrary, including the mucous membranes, the lower or cell-elements are more abundant, and are perpetually discharged to be replaced by new cells. The blood is a fluid composed entirely of non-coherent cells, and therefore belonging to the lowest type of metamorphosis. From it the tissues are formed by the development of its colourless cells or lymph corpuscles through the successive stages of the regular metamorphosis. In corroboration of this doctrine (which we may observe is peculiar to Dr. Addison), he adduces the fact that the earliest stages of cell-growth throughout the body exactly resemble these corpuscules; as may be seen in the cells detached at an early period from mucous structures, and also in pus-corpuscles. In like manner "no wounded or abraded texture heals, and no diseased texture ulcerates, without discharging an abundance of colourless cells;" some of which are, in the former case, carried into the higher grades of development, and go to form tissue; while in the latter, they are all cast off without undergoing development. In both cases a large excess of cellular elements is set free, in virtue of a law of nature, by which any cause producing stimulation of a part produces a flow of blood and a discharge of colourless cells at the seat of irritation. Inflammation therefore and reparative action, consist of the crowding of these colourless cells into a part, and their subsequent metamorphosis into higher types of structure. They are thus exaggerations of normal processes of growth.

In certain conditions of the system the metamorphosis of tissue takes place *imperfectly*. The colourless cells which are poured out on the surface of wounds (forming granulations), are slow to un-

dergo the change into tissue; in this case the inflammation is called *unhealthy* or *asthenic*. If a similar defect of development accompany the ordinary process of growth, the normal textures become gradually exchanged for or supplanted by, the products of a *retrograde metamorphosis*, and scrofulous disease is developed.

Scrofula, therefore is essentially, according to Dr. Addison, a retrograde metamorphosis of the tissues, in which colourless cells of a low type are substituted for the coherent cells and fibres, or possibly for the cartilages, bones, &c., which form the structures of the perfect human body. This morbid condition may take the form of ulcers, in which the natural tissues become more or less gradually broken down into pus, the cells of which appear to have no power of forming themselves, first into granulations, and then into tissue, as in natural inflammation; or it may result in the formation of tubercles, where the colourless cells are deposited in particular points or centres, from which they spread, and finally break down, leaving cavities or ulcers. The "*vis scrofulosa*," as Dr. Addison calls this tendency, may also manifest itself in the asthenic inflammation following a wound, or the imperfect reparation of a fracture.

"Inflammation, and organic disease, are appropriate terms when new products *arise in addition to*, or upon, the normal textures; for instance—false membranes—exudations—secreting vilii or granulations *on* the pleura; layers of lymph *on* the iris or pia mater; solid matter *in* the air-cells of the lungs; or red fungosities *sprouting from* the conjunctiva of the eye. Scrofula and scrofulous diseases are applicable terms when the new products *destroy* the natural fabric —fungosities *supplanting* the cartilage of the joint, or the hard structure of a bone—ulcerations yielding pus *displacing* the papillæ of mucous membranes—and villous secreting fabrics *destroying* the respiratory parenchyma."

As in the case of plants, the retrograde metamorphosis may be the result of two causes; there may be an original taint of constitution in the subject of it, or this taint may be acquired from the presence of unfavourable external conditions. Once acquired, it may be transmitted to offspring, and may be lost again at a future period.

Such are the outlines of the very remarkable theory of Dr. Addison, which, as the physiologist will at once perceive, contains far too many true and too many doubtful elements to be made the subject of hasty criticism; undeniably bearing, as it does, the traces of profound thought and extensive observation. It is obvious that a difficulty, which presents itself at the outset, is Dr. Addison's repudiation of the *specific* character of the tubercle-corpuscle as described by Lebert, who has decidedly every claim to be considered the highest authority on this subject. And yet we can bear out, from personal examination, the following statement of the author:—

"I have repeatedly examined with the microscope the material deposited in the air-cells of the lung in *pneumonia*, and compared its characters and appearance with that forming a tubercle, with-

out being able to detect any more essential or specific difference between them than exist between purulent matter recently excreted and that of an old chronic abscess. The same class of objects, colourless cells and altered protoplasmatous matter, constitute in both cases the bulk of the material. And were we to imagine the fluid elements of old pus, removed or absorbed, the remaining solid matter would appear to possess the characters of tuberculous matter; the colourless elements of blood—pus and tubercle—passing by insensible gradations into each other."

Whether the above statement be the result of imperfect observation or not, it is certain that tubercle-cells are often distinguished with great difficulty, especially in secreting structures.

Monthly Journal, Oct. 1849, *p.* 1155

43.—*On the Application of Nitrate of Silver to Ulcerations in the Larynx.*—By James Bedingfield, Esq., Stowmarket.—[Mr. Bedingfield states that he recommended the use of nitrate of silver in cases of ulceration of the larynx, in the *London Medical Repository*, so long ago as the year 1814. He still approves of its employment, but the difficulty is to get it efficiently applied. The solid nitrate would be more serviceable, Mr. Bedingfield thinks, but it is almost, if not quite impossible to apply it. We should, however, use the solution if it only afford moderate relief; and Mr. Bedingfield suggests what he thinks to be an improvement upon the method of applying it as recommended by Dr. H. Green, (see Retrospect, vol. XVI., p. 124). He says:]

Instead of fastening the saturated sponge to a piece of whalebone, I would suggest that it should be attached to a hollow tube, pervious at both ends; for although an extraneous body thus introduced into the trachea must necessarily be very exciting, the introduction of an open tube would secure a free respiration. A large elastic male catheter, containing a piece of wire so bent as to give the instrument the requisite curvature, would probably answer the purpose. The root of the tongue might be depressed by a pair of forceps bent at right angles, with which I have frequently succeeded in removing extraneous bodies, both from the pharynx and meatus auditorius externus. The blades slightly separated, so as to pass on each side the epiglottis, into the pharynx, would, perhaps, be found more convenient than the rectangular spatula recommended by Dr. H. Green.

Provincial Medical and Surgical Journal, April 22, 1849, *p.* 461.

44.—*On an Indication for the Performance of Tracheotomy.*—By Dr. G. A. Rees.—[Speaking of an altered movement of the ribs in respiration, which he considers to be often an important symptom, especially in children, Dr. Rees says:]

The altered movement alluded to is observable at the moment of

inspiration, when the ribs, instead of being drawn outwards by the inspiratory muscles, as in healthy respiration, are, owing to the lesion of the lungs, or obstruction of the primary air-passages, forced inwards or backwards, from the external atmospheric pressure, thus diminishing instead of augmenting the transverse diameter of the thorax. Dr. Sibson, in an interesting paper published in a late number of the *Gazette*, refers to this state of things, and says, "the same remarkable phenomenon is observed in cases requiring tracheotomy, when the obstruction to respiration in the larynx is extreme."

[Dr. Rees thinks that this symptom will guide us in deciding upon the question, often difficult, of the propriety of performing tracheotomy. He says:]

In croup and laryngitis, it will inform, to a degree, as to the state of the lungs, and assist the surgeon in deciding as to the propriety of tracheotomy being performed. If the altered movement be considerable, the lungs are yet free, and the incision may afford relief; but if with strongly marked bronchial or laryngeal breathing, the ribs are motionless, effusion has taken place in the pulmonary tissues, (augmenting the volume of these organs, while rendering them impermeable to air), and tracheotomy will be in vain.

Medical Gazette, June 1, 1849, *p.* 961.

45.—*On the Use of Coffee in Hooping-Cough*,—Various medicines have from time to time been brought forward as specifics in hooping-cough; and some of them we have tried. To quinine, salicine, arsenic, and oxide of silver, we have given a fair trial in a considerable number of cases: and the result of this experience is, our belief that all of them, except the last named, possess some power in checking the disease, when it has gone on for some time, and is marked by periodicity. When the hour of the recurrence of the fits comes to be accurately foretold, all of the above-named drugs given in full doses in anticipation of the attack, postpone, or entirely prevent it. In the early stages, or later, where there is some amount of bronchitis, the free use of stimulating embrocations, with occasional expectorants, and stomachics, and alteratives (such as a powder composed of cusparia, trisnitrate of bismuth, and rhubarb), are the means we trust to. When the cough assumes a distinctly periodic character, we recommend a trial of antiperiodic remedies and change of air, both of which measures, in many cases, prove of signal and speedy benefit.

From Pereira's summary of the therapeutic uses of coffee, it is not unlikely, however, to exercise some control in hooping-cough. "It has," says Pereira, "been employed as a febrifuge in intermittents; as a stomachic in some forms of dyspepsia; as an astringent in diarrhœa; and as a stimulant to the cerebro-spinal system in some nervous disorders." Floyer, Dr. Percival, and others, have

used it in spasmodic asthma; and Laennec says, "I have seen several cases in which coffee was really useful."

The recommendation of coffee, in hooping-cough, seems decidedly to merit attention. Dr. Jules Guyot, who advocates the use of the remedy, writes thus, in the Union Médicale for 24th April, 1849:—"*Café à l'eau*, hot, and well sugared, in suitable doses, taken four times, or oftener, daily, will cure, in from two to four days, the most obstinate cases. For a child of two years, the dose is a tea-spoonful; for a child of four years, a dessert spoonful; and for an elder patient, a tablespoonful. To obtain a rapid and permanent cure, it is necessary to conjoin with the coffee treatment a diet of fried and roast meat, taking care to mince it, if the child cannot masticate it sufficiently. The quantity of milk used, ought to be diminished: and farinaceous food, confectionaries, and fruits, must be entirely prohibited."

The alleged specific power of coffee in hooping-cough, was accidentally discovered by observing the following case:

A child of four years old, under rigid dietetic treatment for measles, was seized with congestion of the lungs, against which energetic antiphlogistic treatment was put in force. When Dr. Guyot was called in, death seemed imminent from violent paroxysms of cough, which induced such alarming suffocation and syncope, as to cause the patient to appear to be actually dead. Some spoonfuls of strong beef-tea, with some spoonfuls of sweet and hot infusion of the lime-tree, were prescribed. The child passed the night without fainting. On the following day, the same infusions were continued, and in addition, there was given a little bit of grilled and finely minced mutton, with the view of restoring strength as rapidly as possible: but a severe fit of coughing came on, which terminating in vomiting, caused ejection of the food: recourse was again had to the beef-tea, which provoked a similar fit of coughing, and was ejected. In these circumstances, with death apparently impending, a tea-spoonful of coffee was given after each tea-spoonful of beef-tea. Not only was the beef-tea retained: but, to my great surprise, the cough likewise ceased. Two hours afterwards, the child took with relish a little bit of hashed cutlet, followed by a tea-spoonful of the coffee: the food was not ejected, digestion went on naturally the night was passed in comfortable sleep, there was no more cough: —and in truth, the child was saved. On the following day, the treatment was continued, and there was a continuance of a like satisfactory state. On the third day, however, the relations omitted the coffee, when the cough returned, after the first meal, with all the characteristic violence of well-formed hooping-cough: and this state continued during the day. On the fourth day, when the coffee was resumed, the cough disappeared. During forty-seven days the coffee was persisted in, when the cure was distinctly complete. The little patient was the only daughter of M. Haquin, a master boot-maker of Argenteuil.

This case was furnishing subject of serious reflection, when Dr. Guyot met in a public conveyance M. Bouju, ex-notary of Fran-

conville: he told him that he had been obliged to have double doors for his study, to keep out the terrible and incessant noise caused by his two children with hooping-cough. Dr. Guyot detailed the case above narrated: and he cured both his children in four days by means of coffee. He has since tried the remedy successfully in above sixty cases. The efficacy of coffee in hooping-cough seems to show that the seat of the disease is not in the bronchial tubes or larynx—nor in their vascular or nervous network—but exclusively in the digestive organs, and especially in the pharynx and stomach. The cough and convulsive movements of the larynx are excited by a pruriginous irritation of the pharynx; which, again, probably depends on some special affection of the stomach.

London Journal of Medicine, August, 1849, *p.* 772.

ORGANS OF DIGESTION.

46.—ON CERTAIN STOMACH AND NERVOUS DISORDERS, AS CONNECTED WITH THE OXALIC DIATHESIS.

By Dr. James Begbie, Edinburgh.

There is a class of patients,—a numerous class it is,—composed chiefly of those in the prime of life, and including more males than females. They are the subjects generally of the sanguine or melancholic temperament; men unaccustomed to laborious or active exertion, usually belonging to the upper classes, and accustomed to indulge in the good things, particularly the sweets of the table. They present themselves as sufferers from indigestion, from its mildest to some of its most aggravated forms; often with no disorder apparently, but the uneasiness arising from imperfect digestion, and languid assimilation, a sense of load at the pit of the stomach, with flatulence and palpitation some hours after a meal. Oftener, however, they have sufferings of a more severe kind, referable not to the digestive organs alone, but influencing very seriously the nervous system, and threatening the mental condition of the patient. They are usually peevish, sensitive, and irritable, or dull, desponding, and melancholic. At times full of gloomy fears and dark forebodings, they are painfully distrustful of their own powers; suspicious of some lurking disease,—especially pointing to consumption or disease of the heart, as the source of their apprehensions; and from the dread of some threatened evil, they are not unfrequently hurried to the brink of mental derangement. In the milder forms, there is in these patients the anxious countenance and the look of ill health, a loaded tongue, a dry skin, and an irritable pulse. In the more confirmed, a dingy dark complexion, perhaps a red and tender tongue, with growing emaciation, falling off of the hair, eruptions of the scaly kind, sometimes of

boils or carbuncles, pain of a dull but deep-seated kind in the back and loins, hemorrhage from the bowels and bladder, incontinence of urine, and complete prostration of the virile powers.

[These symptoms, Dr. Begbie observes, are very generally connected with the oxalic diathesis, as shown by the presence of oxalate of lime in the urine. He gives the following cases as illustrations of the symptoms, and of the treatment to be adopted:]

A clergyman, aged 36, of sanguine temperament, and spare habit, studious and devoted to his profession, which he had exercised for many years in North America, applied to me at the close of October, 1845, for the relief of symptoms of stomach affection, under which he had laboured for many years. He was highly dyspeptic and nervous, and in his adopted country had been treated actively for "bile" and "liver" and had undergone, by different hands, a variety of prescriptions, from the mild anti-bilious pill to the large and salivating doses of mercury, with only one effect,—the aggravation of his sufferings. After long trial, and many disappointments, he was forced to relinquish his calling, and return to Scotland, in shattered health, and broken spirits. On his arrival, at the date referred to, he was pale and emaciated, and indications of mental and bodily suffering were discernable in his countenance and manner. His appetite was depraved, and he ate largely. He suffered from flatulent distension, and irregular beating of the heart; from langour and lowness of spirits; from restless nights, and troubled dreams; from unrefreshing sleep, and anxious awakings. He was incapable in mental application, and had well nigh abandoned the hope of restoration to his duties, and to health. His skin was dry and parched; his tongue loaded; his bowels irregular, and the dejections unhealthy. His urine, which hitherto had not been examined, was of a deep amber colour, and perfectly transparent, possessing a peculiarly rich smell, and a sp. gr. of 1·028. It was of acid reaction, slightly phosphatic, and contained urea in excess. Under the microscope, which alone can determine satisfactorily the presence of the oxalate of lime, a large number of the characteristic crystals of that deposit were discovered, and the treatment pursued was in accordance with the principles and practice maintained by Dr. Prout, in the management of that diathesis. The bowels were cleared by repeated doses of gentle laxatives; the diet was regulated so as to exclude, as much as possible, all saccharine materials,—and milk, farinaceous vegetables, and animal food in small quantity, enjoined. Tepid sponging and warm clothing were also recommended, and the nitro-muriatic acid in doses of twenty drops twice or thrice a day, prescribed. I have generally adhered to the following formula, omitting occasionally the syrup and increasing the water.

℞. Acid. nitric. dil.; acid. muriatic. dil; syrup. aurant. āā ℥ss.; aquæ ℥iss. Misce.

Sig.—A tea-spoonful, mixed in a wine-glassful of water, before meals.

On the 25th of November, nearly four weeks from the commencement of this mode of treatment, the urine was again examined and found of sp. gr. 1·024, while it maintained the former characters, both under the eye, in glass vessels, and under the microscope. The general appearance of the patient, however, was somewhat improved, and the dyspeptic and nervous symptoms were less distressing. A steady perseverance in the same mode of treatment was enjoined. On the 18th December, the urine was of a straw colour; of acid reaction, of sp. gr. 1·024; unaltered by heat or nitric acid, and presenting under the microscope fewer crystals of the oxalate of lime, together with some of those of lithic acid. The symptoms in general were mitigated; but as the patient complained of gastralgia, and the urine was more decidedly acid, the former prescription was intermitted, and ten grains of the white bismuth ordered to be taken three times a-day; and the same attention to diet, regimen, bathing, and clothing, as heretofore, continued. In the beginning of January the nitro-muriatic acid was resumed. On the 31st the urine brought for examination contained a considerable deposit of the lithate of ammonia, and readily cleared on the application of heat. It was still of high specific gravity, 1·025, and of acid reaction; while under the microscope the crystals of the oxalate of lime were fewer in number, and less in size. The nitro-muriatic acid was again suspended. Digestion being now performed with less uneasiness, and the general appearance of the patient improved, while his nervous symptoms were much abated, he was allowed, at his own request, to occupy the pulpit of a brother minister from time to time; and finding that he could satisfactorily discharge such duties, his spirits and health corresponding advanced.

A gentleman, aged 35, attached to no profession, accustomed to regular exercise, and rather luxurious in his habits; of sanguine temperament and strumous habit; about the beginning of 1844, according to his own account, began to complain of indigestion, characterised by loss of appetite and strength, by frequent perspirations over the forehead, on making slight muscular exertions, and by increasing emaciation. A papular eruption appeared on the chest and between the shoulders, and continued more or less for eighteen months. This was accompanied by an uneasy sense of weight at the pit of the stomach, loaded tongue, restless unrefreshing sleep, and great anxiety of mind. At this time he left Edinburgh, with the view of a permanent residence in the north of England, and in the hope of the enjoyment of better health. He was, however, disappointed, and after a trial of five months of the climate and practitioners of the south, he returned to his former home, depressed in mind and wasted in body. I saw him towards the end of October, 1845, a miserable dyspeptic, and hypochondriac; suffering from headache, flatulence, and palpitation; from constant restlessness of mind, and painful anxiety regarding the future; ever desirous of change, but never attaining the desired haven. He was particularly suspicious of the presence of con-

sumption, or disease of the heart. His tongue was furred; his bowels constipated; the evacuations discoloured, and frequently mixed with blood. He complained of pain in the region of the liver, and of numbness of the right arm. His skin was dry and parched, his eyes suffused and painful, and his hair falling out. He complained of a "lanky feeling" about the loins and abdomen, of uneasiness in the region of the bladder, and of the loss of all sexual desire. His urine, at this time, was of high amber colour, and of sp. gr. 1·040; of acid reaction, and with excess of urea. Under the microscope there were large numbers of the octohedral crystals of the oxalate of lime and much epithelial matter. He was ordered repeated doses of castor oil, a simple diet of farinaceous food, warm clothing, tepid sponging of the skin, and the nitro-muriatic acid. On the 25th of November, the urine was of sp. gr. 1·036, and still loaded with numerous crystals of the oxalate of lime. The general symptoms had undergone little change. On the 18th of December the urine was loaded with the lithate of ammonia, and the crystals of the oxalate of lime were less numerous, the sp. gr. was 1·030: the dyspeptic and nervous symptoms were less severe. He was ordered to intermit the acids, and to use an infusion of snake root, with small doses of the carbonate of potass. On the 8th of January, 1846, the crystalline deposit, as seen under the microscope, was still undiminished, and was mixed with a few crystals of lithic acid. There was little amendment in the general symptoms. The snake root was intermitted, and the nitro-muriatic acid resumed. On the 22nd of February, the oxalate of lime had considerably diminished, and the lithic acid increased, the bowels were more easily managed, the tongue had improved, and the dyspeptic and nervous symptoms were less urgent. He was ordered to alternate monthly, the use of the acids and the vegetable infusion. He was ordered to persevere, and on the occurrence of warm weather to seek the country, and country sports. On Aug. 3rd, the sp. gr. of the urine was 1·026, it was of straw colour, and contained numerous crystals of lithic acid, and a few only of oxalate of lime. The acid was intermitted for two months. On the 12th November the lithic acid had disappeared, but the octohedral crystals were present in considerable numbers and of large size. The acid was resumed. During November and December the oxalate diminished in number and size, the patient gradually improved in looks and spirits; he regained his lost powers; his skin became soft and smooth; the blood disappeared from his stools; his manner and conduct indicated that he no longer thought *only* of himself and his ailments, and in the course of a few more months, under the cod liver oil, he regained his lost flesh, and acquired new health. He has long since ceased his attendance on me, but I have frequent opportunities of observing his studious observance of those rules of diet, regimen, and exercise, which I hope will exempt him from the perils and sufferings of the oxalic diathesis.

It has been alleged, and I apprehend that the opinion is very generally entertained, that the deposit in the urine of the oxalate of lime is of rare occurrence, and that the morbid condition of the blood in

which it originates cannot be considered as identified with the ordinary forms of indigestion. On the contrary, I am clearly of opinion, that the deposit in question is of very frequent appearance, and that its continuance in the urine marks a peculiar diathesis which must have resulted from a mal-assimilation, characterised by more or less of that train of symptoms detailed in the foregoing histories. These cases occurred to me in practice along with several others, all within a short period of each other; and taking a retrospective view of the sufferers from stomach and nervous complaints by whom I have been consulted during the last five years, I find that the large proportion of them have been the subjects of the oxalic diathesis; and that the dyspeptic and hypochondriac symptoms have yielded and disappeared, just in proportion to the diminution and retirement of the oxalate of lime in the urine. The indigestion and mal-assimilation which result in the *lithic acid diathesis*, and which are exemplified in gout and rheumatism, in febrile and inflammatory ailments, are, no doubt, of more frequent occurrence, but it is comparatively seldom that we have to deal with these as a form of dyspepsia, the primary disorder being generally overlooked in the more pressing urgency of other symptoms which follow in its train. The symptoms, again, which denote the *phosphatic diathesis*, though generally regarded as belonging to indigestion, are by no means so marked, or so common as those connected with the oxalic poison, and are characterised by more serious disorder of the brain and spinal marrow. Many of the most obstinate and severe headaches which we are called to treat in young women, and in those of a certain age, and which are usually regarded as nervous and dyspeptic, are dependent on this diathesis, and yield only to a plan of treatment by which the mal-assimilation is corrected, the blood purified, and the earthy salts removed from the urine.

Monthly Journal, August 1849, *p.* 943.

47.—ON THE TREATMENT OF TROPICAL DYSENTERY BY ENEMATA OF TEPID WATER.

By E. Hare, Esq., 7th Bengal Irregular Cavalry.

[Mr. Hare states, that having followed the mode of treatment here described, with marked success for five years, he was induced to make it known to his medical brethren in India, and has found that the results of their experience in its use are confirmatory of his own. He says:]

Dr. Annesley's description of dysentery is as follows:—

"The view we have given of disorders, which depend on accumulations of morbid secretions and local matters in the cavity of the larger bowels, shows in a remarkable manner, one of the very earliest stages of dysentery. Collections of excrementitious matters tend very evidently to irritate and inflame the mucous surface on which they lodge, and cause ulceration and even sphacelation in

a very short period if neglected, or injudiciously treated. In a great many cases, dysentery is preceded by costive bowels, often of long duration. In a few instances, when the evacuations are copious, the diarrhœa subsides, and the patient escapes, at least for that time, a true dysenteric attack. This result seems to arise from the irritation produced in the bowels by the fæcal accumulations having subsided, in consequence of the irritating matters having been removed. Frequently the dysenteric symptoms are present from the first, the stools being scanty, and streaked with blood, and with abdominal pain and tenesmus. In cases of this nature, the increased action of the muscular coats of the bowel, to which may be added the swollen state of the mucous membrane, especially about the sigmoid flexure and rectum, prevents the passage of the fœcal collections through their canal, and in many cases occasions complete obstuction, little passing away, but the perfect fluid secretions. In cases of this description, if the disease be not early subdued by very decided treatment, sloughing of the mucous coat takes place, followed by involuntary motions, when the fæcal accumulations at last come away, such parts of them at least as have been dissolved, being washed off by the watery secretions poured from the irritated vessels of the inflamed surface."

This is the result of Dr. Annesley's large experience, and in most expressive language he shows the fatal effects of accumulations of fæces and acrid secretions in dysentery, and that the danger of this disease arises solely from their retention. This is corroborated by Dr. Gregory, Bampfield, and a host of others in equally large practice.

[Mr. Hare says that every writer on the subject of dysentery with the exception only of Dr. Raleigh, is of the same opinion. He gives the following account of a case by Dr. Annesley:]

"At first, purgatives brought away large fæculent stools; but watery evacuations continuing, Dr. A. supposed that there was still lodgment of fæculent matter somewhere in the intestines, and that the watery evacuations were the effect of the irritation caused by its acrid qualities. Large doses of purgatives were therefore continued for several days, but no fæces came away. On his death, the small intestines were found loaded with fæculent matter. The purgatives had brought copiously away at first the fæces from the large intestines, so that, before this evacuation, the man's abdomen was crammed with fæces, both large and small intestines."

Can there be a mistake here? And this was in India. But why multiply similar examples which may be found, not assertions, but *facts, cases*, in every writer on dysentery, from Galen till now. I cannot account for Dr. Raleigh's assertion, and it is vain to try; but I think, that where the accumulations are not very large, it is probable that nature herself, by throwing out fluid secretions, softens down and evacuates them; but in doing this, the gut is ulcerated and destroyed, though on dissection it is found empty. Dr. Annesley says:—

"The reason why scybala and solid fæces are seldom found in East Indian dysentery, is that the solid serous fluid copiously secreted from the irritated intestine washes it, squeezed, as it were, through the constricted canal in a liquid form."

These views, as to the retention of the excremental matter in dysentery, are elucidated and confirmed by the case of Dr James Johnson, in Calcutta. Nothing relieved the symptoms till the calomel he took procured a copious *fœculent* bilious stool, succeeded, he says, "by such delightful feelings, that I prayed aloud." How then can Raleigh assert, that solid fæcal matters are *never* found in the acute dysentery of this country? I suspect, that in almost all that die, the efforts of nature have already softened and carried off in fluid the solid matters, and that Dr. R. would have got rid of these in the solid form, and saved often the lives of his patients, had he given purgatives more freely than he did; for he gives very small doses compared with others. If dysentery attack a man with costive bowels, (and one-third of Europeans in this country have very costive bowels), there *must* be fæcal matter, and very often scybala in the intestines, which from the nature of dysentery, cannot be expelled without nature's most painful efforts. I do not assert that dysentery is caused by accumulations; I think it rarely is. But when caused, the fæces it finds in the intestine, and the irritating secretions it afterwards adds, are the principal, perhaps *only* cause, of its destructive termination, and I have no doubt that the danger of dysentery is essentially caused by the long contact of fæces and acrid secretions with the inflamed surface of the intestine; and if these be freely removed, the danger at once ceases.

No author except Raleigh says that solid fæces are never found; and any one that doubts that they are not almost *always* found, and in large quantity too, had better consult Annesley's plates of dysentery, which are not selected to illustrate my theory. He will find them *all*, and without exception, plates of fæcal accumulations, sometimes in large lumps here and there. Sometimes the intestine is so crammed, that the horizontal colon is bent by its own weight, down from the stomach to the region of the bladder.

I have shown that all writers on dysentery tried their best to empty the bowels by purgatives, as the one grand object in its treatment; for, this not accomplished, vain are their efforts to subdue the inflammation. Suppose an eye inflamed from cold or otherwise, put a lump of dysenteric fæces within the lid, and shut it up there, do you think that any bleeding, or 20 grain doses of calomel, will prevent ulceration of the coats? This may be a coarse argument, but it is true, till it can be proved that one part of the great mucous membrane, continued in one piece from the eye to the anus, differs anywhere in its nature. What would you do in such a case? why, wash out the filth first, and then the inflammation will subside of itself, or with very moderate depletion.

[The treatment advocated by Mr. Hare in acute dysentery, is, then, the injection of warm water by means of an elastic tube

passed above the sigmoid flexure, as recommended some years ago, for other diseases as well as dysentery, by Dr. O'Beirne. Mr. Hare says:]

By passing the elastic tube above the sigmoid flexure, you can inject from four to six pints, and thoroughly wash out the intestine from cœcum to anus.

It is satisfactorily proved in the work of Dr. O'Beirne, that the chief obstruction to the fæces always is in the sigmoid flexure, the rectum and itself remaining almost empty both in health and disease. The fæces accumulate, therefore, above the sigmoid flexure, the twistings of which, narrowed as it is in dysentery by spasm, and the swollen state of the mucous membrane, form so complete an obstruction, that nothing but fluid, and that with difficulty, can pass. The accumulation itself, too, greatly increases its own obstruction, by impeding the circulation of the mucous membrane below it, almost to strangulation, and causing sometimes so much swelling, that the mucous membrane protrudes through the anus, causing prolapsus, Now, Bampfield and Annesley, with their syringe, had no means of injecting more than the rectum, and a little, perhaps, of the sigmoid flexure; but they could never reach, through the obstructed sigmoid, the fæces beyond. The irritable rectum, too, on the slightest distension, contracts on and expels the injection. Even in health, if a small quantity of fæces pass into the rectum, it produces an instant desire to stool; but large quantities may accumulate for weeeks above it without causing this sensation.

If, then, a tube can be passed directly into the fæcal mass, above the sigmoid flexure, this feeling, on injecting, will not be immediately felt, and the water, by the pumping action, being intimately mixed with the fæces, it is evident that the injection cannot return without fæcal matter with it. It is astonishing how readily a hard mass of fæces will soften and break down in water. Any one may prove this for himself without much trouble. The tube, too, can be moved up and down, and the water made to wash its way, at all points of the descending colon. By the first injection, some fæcal matter, at least, will be softened and removed, and the injection can be applied again and again, till the relieved intestine eject itself the harder matters, reduced, as they must be, in size, if any remain; and this, in fact, is the way in which purgatives evacuate the fæces in dysentery, having first reduced them to a soft or watery consistence by the fluid secretions they elicit from the intestines; but what increased irritation must they cause in doing this.

Chronic Dysentery.—If injections are valuable in acute, still more are they in chronic dysentery. This disease Annesley describes in the following terms:—"The inflammation of the acute stage leaves the villous coat of the colon throughout hypertrophied. Its papillæ having an erect rough appearance and feel, of a dark red or purplish hue, with here and there livid patches of congestion, particularly at the caput coli, and transverse arch, where ulcers are often

found. The sigmoid flexure is often very much contracted, its villous coat of unnatural thickness, and inflamed as well as the rectum. The lining membrane of the small bowels for some way up, is in much the same state, ash-coloured ulcers, &c. &c."

Now, I ask, is this a condition of the intestine likely to be improved by the irritation of purgatives, and yet the contraction of the colon, particularly the sigmoid flexure, together with the loss of muscular power and peristaltic action, necessarily succeeding the acute stage of the inflammation, render it absolutely necessary that they, or some substitute, should be given. The secretions, too, continue acrid, the appetite is often ravenous, while the powers of digestion are much weakened. The half-digested food and acrid secretions accumulate and distend the intestine above the contractions of the colon, causing dreadful irritation, and constant purging of fluid matters. Purgatives have, therefore, at all times been unwillingly resorted to, as a necessary evil; every writer inculcating in theory the use of the mildest laxatives, but in practice, in their *cases*, using from necessity calomel, epsom salts, black draughts, and compound jalap powder. To correct these, they used freely opiates and astringents, which, in checking the dangerous symptoms of the purging, produce accumulations. The accumulations require again purgatives; and so on the changes are rung, till the patient dies, or escapes only by the powers of a strong constitution; which, nevertheless, is often weakened, if not destroyed, by the previous treatment, for the acute stage.

See how dissatisfied Bampfield is under this destiny:—"It must be granted that astringents display great powers over the morbidly secreting vessels; but it must also be admitted, that if they induce constipation, the morbid secretions are much increased, and it becomes necessary to abandon them. If the constipating effects of these astringents could be generally obviated, by the combination of a defined quantity of any purgative medicine, which would not decrease their astringent effects on the morbidly acting vessels, then it would appear, that the desideratum for the cure of this disease would be obtained."

Since 1822 Dr. O'Beirne has discovered this desideratum; yet no one has thought of making use of the method, though it is infinitely preferable to this half purgative half astringent system, proposed here by Bampfield (if it were possible, which it is not). By the introduction of the elastic tube, you may wash out daily, with the most soothing applications, warm water, or milk and water, the excoriated intestine, removing its acrid secretions, and the fermenting, half-digested fæces, fomenting its tender surface the meanwhile, and softly stretching the strictured parts, with the gentle expansion of water. And when in the intestine, thus daily cleansed and soothed, the inflammation and irritation have been calmed, what numerous applications may be conveyed by the tube to the whole diseased surface of the colon, from the cœcum to the anus, gently to constringe the overstrained vessels, and heal the

ulcers, the suggestion of the effect of these medicines, in disease of the mucous membrane of the eye, will be sufficient proof.

No constipation can be caused in this method of injecting, or even by giving these astringent medicines by the mouth, for the fæces ought to be daily removed by a large injection of water, previously to the exhibition of the small astringent injection, or astringent medicine by mouth, and great care ought to be taken, that a sufficiently large injection be daily thrown up to empty the bowels completely.

[The proper quantity of injection is three or four pints, though Mr. Hare observes, the living intestine would hold a much larger quantity. Mr. H. proceeds to say:]

The astringent injections I have found most useful, have been nitrate of silver, fifteen grains, dissolved in two and a-half to three pints of water, a strong decoction of bark, and a mixture of catechu and chalk. The first and last I have found particularly useful. No doubt the nitrate of silver might be increased much in strength, but I have not been able yet to ascertain how much, from a dislike to try dangerous experiments without extreme caution. It is absolutely necessary to clear out the bowels first with a large injection, before injecting the medicinal one, which may be smaller.

I believe that inflammation of the mucous membrane of the colon ought to be treated on exactly the same principles as in the conjunctiva, where salivation is now rarely trusted to, but great use made of astringent lotions, with moderate depletion, and great attention to cleanliness, frequently washing away secretions by warm water or some mild lotion.

Free venesection will often be required in acute dysentery, and leeches in chronic, but very seldom, I think, mercury, though even this might be used as an ointment. It may be as well to observe, too, that it is by no means necessary, in all cases, to introduce the elastic tube far up the intestine; though really the inconvenience to the patient attending it, even in the worst cases, is so trifling, as to make it almost a matter of indifference, if carefully done. After the first clearing of the intestine, a small distance, four or five inches, will suffice, and, latterly, the common short rectum pipe, but the pump is indispensable; the common squirt will not do; and it is most requisite always to inject a full quantity, or we may fancy the intestine empty when it is not. A gentle mulling of the abdomen with the hand, while the injection is still in the bowels, assists it greatly in bringing away the accumulations. I generally keep the patient on his back, occasionally turning him to the left side. When he complains of and I see that there is, distension of the abdomen, I cease to inject, and commence, if he can bear it, gentle mulling. The patient then sits up, and the injection never fails to carry all away with it.

The substance, then, of the whole argument is this. The long tube changes a huge internal abscess into an external, and enables us to wash out and cleanse from it its putrid contents. It also enables

us to foment and soothe, by *local* applications, the sloughing and ulceration these contents have caused on its surface. If any one can deny the utility of these means, I confess I cannot understand him. The consideration, too, that this abscess is in a vital organ, quickly prone to ulceration and sloughing, and moreover that its *function* is absorption, and therefore that it will rapidly absorb its own putrid contents, adds doubly to the force of the argument.

On those who may in future try this system, I would strongly urge, not to mix the old treatment with it, and dose their patients with calomel and purgatives which are directly opposed to it, and not to be led away by false theories of hydropathy and absorption of water, which are utterly without the shadow of probability, and to give the injections without limit in quantity but the patient's endurance. You cannot injure a piece of dead intestine by distending it with water, much less the living, which quickly contracts powerfully on its contents; and do not cease repeating the injection till you are satisfied that the colon is evacuated and cleansed.

Edinburgh Medical and Surgical Journal, July 1849, *p.* 26.

48.—*On the Uses of Sub-Nitrate of Bismuth in Gastro-Intestinal Affections.*—By M. Monneret.—M. Monneret states that he has been for some years engaged in experimenting upon the medicinal qualities of this substance, and he finds that, given in far larger doses than are supposed to be permissible, it is of the greatest value in gastro-intestinal affections, especially those attended with fluxes.

Simple Diarrhœa.—It is especially in the diarrhœa of infants, resulting from imperfect lactation, that he has found it so very useful, large doses curing such in a few days. So, too, after weaning, and the injudicious diet so often adopted, the bismuth, in doses of three tea-spoonfuls a-day, soon removes all symptoms of disordered digestion, especially the serous diarrhœa so often produced in these cases. Simple diarrhœa is of much rarer occurrence in the adult; still, examples are met with in which we cannot attribute it to either phlegmasia or ulceration of the intestines, as in the granular disease of the kidneys, in chlorotic women, in women become anemic from cancerous degeneration of the uterus, and in some cases of chronic disease of the heart, or in commencing phthisis. Those cases in which the diarrhœa is serous, and results from an atonic state of the gastro-intestinal membranes, best yield to this medicine. It should be given in gradually increasing quantities from two drachms to twelve or more daily

Diarrhœa Symptomatic of Intestinal Lesion.—In spite of the presence of symptoms indicative of inflammatory action, the author has frequently administered the bismuth to children with great success. So, too, he has found it very useful in some subjects of phthisis suffering from obstinate colliquative diarrhœa, and even in the diarrhœa of typhoid fever, when the intestinal canal was certainly the seat of very numerous ulcerations. In this last case

its utility is not very permanent; but it has the advantage of inducing the tolerance of articles of diet or medicine, which without it would be rejected.

Cholerine.—The frequency with which gastro-intestinal disturbance precedes true cholera is well known, and the desirability of its suppression is evident; and to this end large doses of bismuth succeed better than any other means. It is most suitable in the cases in which with the diarrhœa there are nausea, vomiting, gastralgia, colicky pains, borborygmi, and anorexia. The cases are innumerable in which the author has been enabled rapidly to relieve these symptoms, without any aid from opium, by giving eight or ten drachms daily. The only inconvenience resulting is a certain amount of constipation.

Gastralgia.—Various observers have already admitted the great utility of this substance, both in idiopathic gastralgia and in the forms dependent upon hysteria, hypochondriasis, chlorosis, &c. &c., but it has frequently failed in the hands of others, by being given in doses of two or three scruples daily, in place of twelve or fifteen drachms; and it is taken with such ease, that the patients voluntarily continue its employment after the pain has ceased, until the gastro-intestinal functions are thoroughly re-established. The idiopathic forms of gastralgia, so often met with in sedentary or literary persons, and in those suffering from depressing emotions, or from abuse of alcohol or coffee, are those which are most successfully combated. It has often, too, enabled the stomach to bear those tonic aliments, whose slow and painful digestion accompanied by cephalalgia, drowsiness, and pain in the back and limbs, is so often observed in chloro-anemic patients. This medicine only produces a temporary relief in gastralgia, dependent upon organic disease of the stomach, except when this viscus secretes a large quantity of acid fluid.

Vomiting.—Vomiting may depend upon a simple gastric neurosis, and then the bismuth can be very usefully employed. It is also useful in the vomiting of pregnancy, and that accompanying dysmenorrhœa; but its efficacy is less certain than in affections of the gastro-intestinal tube, and is never so great as when diarrhœa, colic, and flatus are present. From whatever cause pain manifests itself during digestion, we may relieve it by mixing the sub-nitrate freely with the articles of food.

Administration and Action.—It should be given in powder, and best so with the first spoonful of broth or gruel. It excites no disgust, especially if placed between two bits of bread, soaked in broth. Children take it readily with milk or ptisans. The author has never given less than from two to three drachms daily, nor more than twenty, and he has never observed the slightest inconvenience from these large doses; and it is his custom to give it to the children in his hospital by spoonfuls or table spoonfuls, without observing more exactitude, so innocuous is it. So imperfectly is this fact known that the chemists hesitate in preparing his prescriptions in which these large doses are ordered. The author

cannot conceive why this substance was ever set down as an irritant poison, as he has never found the slightest irritation from the largest doses given to either the healthy or the sick; and post-mortem examination proves that, beyond patches of black discoloration, it produces no effect on the mucous membrane, the consistence of this remaining quite normal. The action of the substance on the canal seems to be quite negative—that is to say, the abnormal symptoms for which it was prescribed quickly disappear. Great attention has failed also to detect any marked effect upon any other part of the system. Perhaps the urine is somewhat increased, and the pulse becomes slower; this is probably due to the relief of the gastro-intestinal affection. The action of bismuth is, then, purely a *local* one ; and this local action, so far from being an irritant one, as so commonly stated, diminishes the activity of the phenomena of which the mucous membrane is the seat.—*Gaz. Méd.* Nos. 15, 16; and *Med. Chir. Rev.*, July, 1849.

Monthly Retrospect, Sept., 1849, *p.* 184.

49.—*Use of Kreasote in Salivation.*—By Dr. Faulcon.—Dr. Faulcon relates a case of profuse mercurial salivation, in which, after the unsuccessful employment of the usual remedies, he employed with great advantage a gargle composed of creosote $3\frac{1}{2}$, sage tea a pint, the affection quickly yielding.—*Phil. Med. Ex.*, No. 47.

British and Foreign Medico-Chirurgical Review, Oct, 1849, *p.* 564.

URINARY ORGANS.

50.—"BRIGHT'S DISEASE OF THE KIDNEYS," ESSENTIALLY AND PRIMARILY A BLOOD-DISEASE.

By Professor Walshe, University College.

[Prof. Walshe observes that the natural history of "Bright's Disease," affords tolerably sure evidence that it is not primarily a local affection, but originates in the blood. The following are some of the principal facts which support such an opinion:]

1. There is no positive and constant naked-eye peculiarity, in the anatomical conditions of the kidney, in *all* cases where there had been discharge of albuminous urine, and cellular dropsy during life. So true is this, that the most practised observers would hesitate, (and have actually hesitated,) from the inspection of *certain* kidneys, to say whether the case had, during life, been attended with the symptoms of simple chronic nephritis, or of "Bright's disease." Of course, this difficulty of distinction is not common, but it exists with quite sufficient frequency to justify the general proposition, as just stated.

2. Neither is there any microscopic character, always present, positively and unfailingly distinctive between the kidneys of persons who have suffered from simple chronic nephritis, or from "Bright's disease." We have frequently, in this hospital, seen clogging of the tubules with simple exudation, (in the stage of "induration-matter,") in victims of the latter disease; while in others, whose urine had been non-albuminous and alkaline, and in whom no secondary diseases existed, (simple chronic nephritis,) fat existed, in some abundance, along with the same "induration matter."

3. There is no positive and direct relationship, in cases of "Bright's disease," between the amount of albuminous impregnation of the urine and of renal disorganization.

4. There is no positive and direct relationship between the number and severity of the "secondary diseases," and of renal disorganization.

5. The urine may, in "Bright's disease," be occasionally copiously albuminous, without co-existing dropsy; or the patient may be dropsical, while his urine is free from albumen.

6. Albuminous impregnation of the urine is caused by other forms of renal disease proper, besides those met with in "Bright's disease," and also by certain morbid states of the blood.

7. Albumen may totally disappear from the urine, *pro tempore*, in the most serious cases of "Bright's disease," yet it cannot be supposed that the physical conditions of the kidney, favouring filtration of serum through the vessels, have suddenly and completely changed.

8. The reason why there is greater clogging of the tubules (general, or limited and "granular") in the advanced stages of "Bright's disease," (with proto-plastic, or fatty matter, or both,) is, that longer time has been allowed for the morbid blood-elements to filtrate through the walls of the vessels. These elements, in part stagnating within the tubules, in part exuded amid the intertubular tissue, accumulate in the renal texture, instead of all passing away with the urine. These elements, deposited granularly, and by uniform infiltration, are the *effects* of the disease, not its *cause*. But as the alterations of the blood are not equable and uniform in their advancement, there is no direct ratio between the amount of clogging of the tubules, &c. and the severity of the disease, considered in the sum of its general manifestations. So true is this, that (as is clinically well known) the very highest amount of "secondary affections" may exist at the outset (anatomically considered) of the disease of the kidney.

9. From the very outset of "Bright's disease," the existence of blood-changes is certain; they are more marked in degree, and earlier in development (as appreciable conditions) than in any recognized chronic blood-disease.

10. "Bright's disease" has several of the characters of a primary chronic blood-disease. Thus: there is no direct ratio between the anatomical changes and the mass of morbid condition constituting the disease;—"Bright's disease" tends to the production of various

secondary and dependent affections: and the character of these secondary affections partakes of that of the primary (that is, primary in the train of organic changes of the solids) renal changes; they too are marked by transudations of serum, and deposition of proto-plastic matter and fat.

11. It may be said, that as urea has been found in the blood at the commencement of acute attacks of "Bright's disease," there is evidence of organic disease of the kidney *ab initio*. But no; functional inactivity is enough to ensure accumulation of urea in the blood; in cholera, there is no textural alteration of the renal substance. The true explanation seems to be, that the state of the blood prevents the kidney from acting properly on the elements it is accustomed to excrete, not that its own functional aptitude is at the outset seriously impaired; in other words it is probable that at the commencement the renal cells are still quite able to separate urea, if healthily-constituted blood were offered to them by the vessels. This view is, doubtless, hypothetical; but it harmonizes with the well-known and striking fact, that urea may, and does, not uncommonly, disappear from the blood when the anatomical changes in the kidney are carried to extremes, and the case (though a temporary improvement has occurred) is fast tending to a fatal issue. According to the doctrine which regards the accumulation of urea in the blood as the consequence solely of the changes in the kidney, how is this to be explained? According to this doctrine, the effect of a certain cause is decreased as the intensity of that cause is increased.

12. With the progress of deposition within the kidney, the excretory functions of the organ, become, partly mechanically, partly dynamically, interfered with, and hence the blood becomes *secondarily* diseased also.

13. It may be objected that if "Bright's disease" be not essentially a renal, but a blood-disease, *ab initio*, it does not seem clear why the kidney should be the organ on which textural changes are earliest and constantly inflicted. The objection is valueless, until it be explained why tubercle selects the bronchial glands at one period of life for its main nidus, the lungs at another; why cancer chiefly infests the mamma and uterus, &c. *As soon as* the predilections of blood-diseases for certain organs (or the attracting quality of certain organs for special diseased circulating elements) are explained in the instances *inter alia* of tubercle and cancer, the attraction of the morbid material, present in the blood in "Bright's disease," for the kidney will probably be found simple enough. Meanwhile, I would observe, that a fact, which may not be without its application in this matter, was ascertained some years ago by Simon, from the comparative analysis of the blood of the aorta, and of the renal veins in a horse. The quantity of albumen in the former to that in the latter was found to be as 425 : 446. This natural tendency to excess of albumen in the blood of the renal veins may, perhaps, be in some way connected with its tendency to escape by the channel of the kidney in "Bright's disease." The

urine (according to some chemists) naturally contains albumen, but in a proportion so minute that it is undiscoverable by ordinary tests.

From these facts and arguments it appears to me that the following conclusions may fairly be drawn:—

That "Bright's disease" is a blood-disease, *ab initio*, essentially tending to chronicity, though (like cancer and phthisis) sometimes running an acute course.

That the existence of that *ab initio* change follows from—1. Direct analysis. 2. The general accordance of the pathology of the disease with that of chronic blood-diseases. 3. The inadequacy of the local renal states to account for the local renal phenomena, still less for the general phenomena. 4. The total want of harmony between the renal changes and the general implication of the system, and between the renal changes and the local functional disorders. 5. The facility with which that want of harmony is explicable on the hypothesis of blood-disease.

That the precise nature of the primary blood-change is unknown: at present the main peculiarities ascertained (excess of serum of low specific gravity, excess of fat, excess of urea, and deficiency of albumen) are, in all probability, secondary.

That the renal textural changes are not the *cause* of the sum of morbid states, included under the name of "Bright's disease;" and that they do not even furnish a *measure of the intensity* of that disease.

That in reality the renal textural changes form at the most the *anatomical character* of the disease, (and their claim in this respect even is not yet fully made out under all circumstances, *vide* propositions 1 and 2,) just as tuberculous deposit in the lung constitutes the anatomical character of phthisis.

That the primary blood changes are the result, probably, of error in the primary and secondary digestion-processes. The toxic condition of the blood, whatever it is, is not derived from influences acting from without, but generated within the frame through imperfect organic actions.

That, as in all diseases of this class, (rheumatism, gout, tubercle, cancer, &c.) there is a mysterious constitutional *predisposition* to those imperfect organic actions; without this, all the exciting and immediate alleged causes of "Bright's disease" fail in the power to evolve it.

Lancet, July 14, 1849, *p.* 29.

51.—*A new Test for Albumen.*—By M. E. Millon.—The highly acid liquid obtained by dissolving mercury in its own weight of nitric acid, constitutes an extremely delicate reagent for albumen and albuminous compounds.

This mercurial solution communicates to albuminous substances an intensely red colour, by means of which a very minute proportion of albumen in water may be detected.

To give an idea of the delicacy of this reagent, and to show its applicability to the study of vegetable organization, it may be stated that starch and gum acquire by its action a very distinct rose tint. Urine almost always becomes coloured of a rose tint after the nitro-mercurial solution has been mixed with it, and the mixture has been warmed. The albumen of the blood, that of serous effusions, of plants and fibrine, casein, gluten, legumin, silk, wool, feathers, horn, epidermis, gelatin, chondrin, and protein, are equally affected.

Protein rendered soluble by the prolonged action of an alkaline ley, or by sulphuric acid, is also coloured red, but no precipitate is thrown down.

This mercurial solution is most readily prepared by dissolving mercury in its weight of nitric acid (1·4) in the cold. When reaction has ceased, a gentle heat may be applied to facilitate the solution of metal. When the solution is complete, the liquid is to be diluted with two parts of distilled water by measure. After some hours the liquid is to be decanted from any mixed crystals of nitrite and nitrate of mercury, that may subside.

This reagent acts on albuminous substances at low temperatures, but not so completely as at a temperature of from 140° to 150° Fah. It is even preferable to continue the application of heat to the boiling point. The prolonged action of the reagent in excess does not alter the red matter, as has been ascertained by the contact of albumen with the nitro-mercurial liquid for upwards of one year.

According to M. Millon, this singular property of giving a pink or red colour to albuminous substances resides neither in the nitrate nor in the nitrite of mercury, nor in their mixture. It is necessary that there should be hyponitrous acid in the solution which contains the two salts. The pure pernitrate of mercury, saturated with hyponitrous acid, forms a delicate reagent, but inferior to that of a saturated solution of the mixed salts.

One or two drops of the test liquid are sufficient for the detection of albumen. Albumen has been detected in the liquid of cholera, when nitric acid and heat have failed to demonstrate its presence. — *Comptes Rendus.* Janvier 1849.

Medical Gazette, Aug. 10, 1849, *p.* 262.

52.—*On the Diagnosis of Hæmaturia.*—By Dr. Robert Venables, A.M.—[To show the errors of diagnosis into which we may fall by trusting to a superficial examination of the urine, Dr. Venables gives an account of the examination of a specimen of urine, which presented the following characters:]

It was turbid, dark, and quite opaque. Upon standing for some hours, a dark red-looking sediment fell down to the bottom of the jar. The supernatant urine retained no particular colour, remained turbid but allowed the light to pass through. Passed through the filter, it came through of a deep and bright amber colour, and per-

fectly transparent. No peculiar smell. Sp. gr. 1030. Reddened litmus paper.

What was retained on the filter, examined by the microscope, presented a number of epithelial scales, somewhat coloured; some globules of mucus, and a few square tables of lithic acid.

The filtered urine examined for albumen by heat, with both nitric acid, nitro-hydrochloric acid, and prussiate of potass, gave merely a haze, but no trace of this principle. Uric acid separated in very small quantity from the acidulated urine. Solution of sesquicarbonate of ammonia threw down a large quantity of prismatic double phosphate, and also some phosphate of lime.

A portion of the dark reddish sediment, placed under the microscope. was found to consist of very minute rhombs of uric acid of rather a dark outline, agglutinated together, and intermixed with epithelium of a deep colour.

A portion collected from the filter, and ignited, left a residuum which resisted all further action of the heat, even the blow-pipe flame. This residuum, mixed with charcoal, and ignited before the blow-pipe flame,was not attracted by the magnet, and, consequently could not have contained any magnetic iron. The residuum, after ignition, boiled with nitro hydrochloric acid, filtered and diluted with water, and treated with yellow prussiate of potass, gave no appearance whatever of prussian blue. Nor did tincture of galls, or hydrosulphate of ammonia, indicate even a trace of iron. Under these circumstances, I felt warranted in denying the presence of *blood* in this urine.

[This analysis was repeated three several times, with the same result; but Dr. Venables, remembering a case which had occurred to him previously, in which the urine had at first presented similar appearances to those above described, and had afterwards afforded unequivocal indications of the presence of blood, gave a guarded opinion, and directed the patient to send him another specimen of the urine. Accordingly in twelve days afterwards, Dr. Venables]

Received a second specimen of the urine passed by the same gentleman, after active exercise in walking. This was darkish and opaque, but on standing let fall a dense, dark red looking mass, leaving the supernatant urine turbid, semi-opaque, and of rather a brightish red colour. It was nearly devoid of smell; sp. gr. 1030; reddened litmus paper, and abounded, as did the former specimen, in urea, and the earthy phosphates. The presence of blood in this urine was most fully proved by the following evidence:—

A portion of the sediment placed under the microscope clearly showed the blood discs. The urine thrown on the filter, though of the brightish red above described, came through of a light amber colour, without any appearance of red.

A small portion of the matter retained by the filter, collected, transferred to the slide, and placed under the microscope, showed the blood corpuscles, thus proving that the reddish colour of the supernatant urine depended upon the mechanical suspension or intermixture of the red particles.

A portion of the dark red sediment collected and ignited before the blow-pipe, with charcoal, became magnetic, being strongly attracted by the magnet. Another portion ignited; the residue after ignition was boiled with nitro-hydro-chloric acid. The solution diluted with distilled water, and passed through the filter, gave, with the usual tests, distinct and unequivocal evidence of the presence of iron; for instance, with yellow prussiate of potass it formed Prussian blue; tincture of galls threw down the black precipitate; and hydro-sulphate of ammonia threw down sulphuret of iron; free sulphur being precipitated, when hydro-sulphuric acid alone was used.

Assuming, from the above, that in the two cases, the particulars of which have just been stated, that a peculiar condition of the urine—*apparently* but not *really*, sanguineous—preceded actual hæmaturia, it may be matter for future inquiry whether such a condition is necessarily a precursor of bloody urine, or whether it be a precursor only of some particular form or forms? Then the question naturally arises, what peculiar hemorrhagic condition does it indicate as threatened? For myself, the opportunities of observation and for forming inferences have been too limited to enable me to offer anything plausible even, much less satisfactory or conclusive. There is one result, however, of importance, the necessity of not being over hasty in deciding the nature of urinary deposits, without chemically or otherwise determining their properties.

Urine containing blood is almost necessarily albuminous; for we can hardly imagine the blood-discs escaping without the serum; and it was the non-coagulability in the first specimen of the filtered urine, and its *coagulability* and reddish colour previous to filtration in the second, which led me to believe the first to be devoid of blood; and, in the second, to infer the presence of blood and the general sanguineous character of this urine.

Another question also presents here,—the most easy and the most ready means of deciding the sanguineous character of the urinary deposits, and of the urine itself. The elements of the blood may be arranged under the three following heads: viz., the serum, an albuminous fluid not spontaneously coagulable; the liquor sanguinis, a fibrinous fluid spontaneously congulating, in consequence of the spontaneous solidification of the fibrine; and the hæmatine, constituting the red particles. The two first may occur in the urine, as has been frequently observed after certain febrile states of system, especially scarlatina, after which coagulable urine often occurs as a sequela. But we can hardly consider any urine as bloody, unless the red particles of the blood be present; and many instances occur in which the urine, from its general *sensible* properties would be pronounced bloody. The detection, therefore, of the red particles seems essential to be able to pronounce the urine really and unquestionably sanguineous. The coagulability of the urine by heat and certain re-agents, affords, in doubtful cases, but presumptive evidence; consequently, we must look to the hæmatosine as the only certain proof of the sanguineous nature of the urine.

The microscope is much relied on for the detection of the blood discs; but, as it is liable to certain fallacies, and in some instances wholly inapplicable, from changes in the blood discs themselves,—we cannot safely depend upon it. Chemistry offers the next, and probably the most unequivocal means of detecting hæmatine. M. Pariset proposes the following:—Boil and filter the urine, when brown coagulums of albumen and hæmatosine will be retained by the filter. Pour upon these a solution of potass; a greenish fluid will pass through, if hæmatosine were present. If hydrochloric acid be added to the filtered liquor, it will precipitate coagulums of white proteine. This, however, is liable to certain fallacies, which it would be foreign here to our purpose to discuss. It will be necessary to understand the principles of diagnostics in the previous detail, to state the chemical composition of hæmatosine. Mulder, from his analysis deduced the following formula for hæmatosine:—

$$N_3 \quad H_{22} \quad C_{44} \quad O_5 \quad Fe_1$$

The iron in hæmatosine is in the ratio of 6·64 or 6·66 per cent. As iron is an essential principle in the constitution of hæmatosine, it necessarily follows, that if blood be effused into the urine, we shall be able to discover some trace of iron; and this is the foundation of the pathological views enunciated in the preceding observations.

Various methods may be proposed for the detection of the iron; but perhaps the most simple consists in boiling the urine, and coagulating all the principles coagulable by a temperature of 212° Ft. It may next be filtered, and what is retained by the filter should be exposed for some time to a red heat in a platinum crucible, or one of Berlin ware. By this means all the organic matters, and other volatile principles, will be dissipated. The residue may then be mixed with charcoal or black flux, and ignited before the blow-pipe. The iron by these means will be reduced, and rendered magnetic, that is, attractable by the magnet. We thus determine the presence of a principle capable of being rendered attractable by the magnet.

If the ignited or reduced principle be now suspended in distilled water, and a current of chlorine passed through, a chloride of iron will be found in solution, which, on exposure to the air, will pass into sesquichloride.

The ignited mass will consist of oxide of iron mixed with certain fixed salts, as phosphates, &c. From these the iron may be separated by the magnet; but this manipulation will not be necessary.

As the transmission of chlorine may be difficult to some, every object may be attained by digesting the magnetic particles, or even the entire residue, in hydrochloric acid. A chloride of iron will be formed easily convertible into sesquichloride, by boiling with a small quantity of nitric acid. The sesquichloride of iron may be formed at once, by boiling with nitro-hydrochloric acid, prepared by mixing three parts of hydrochloric with one of nitric acid. The solution should be mixed with distilled water, and passed through the filter.

If the iron be in the state of protosalt, ammonia produces a white

precipitate, which soon passes to bluish green. Yellow prussiate of potass, a precipitate, at first white, but which soon becomes blue. Hydrosulphuric acid has no action; but if, after passing through a current of the gas, ammonia be added, a black protosulphuret of iron precipitates. The most characteristic test is a solution of red prussiate of potass, which gives Prussian blue. This does not act on the per or sesqui-salts.

If the iron be in the state of a persalt, ammonia throws down a brownish-red precipitate of hydrated sesquioxide; solution of yellow prussiate of potass immediately gives rise to the formation of Prussian blue. Sulphocyanide of potassium strikes a deep blood-red colour; but there is no precipitate. Tannin, or tincture of galls, gives the solution of iron a deep violet or black colour. A current of hydrosulphuric acid is attended with the separation of a white precipitate, which is free sulphur. If to the filtered fluid ammonia be now added, the black sesquisulphuret of iron falls down. The presence of iron, even in the most minute quantity, may be thus determined; but, where the quantity is exceedingly minute, the evidence of its presence will not be immediate, and will probably require several hours for its complete development.

I have been diffuse in these observations, because I think the matter as important as it is interesting, more especially as relates to diagnostics. I have no doubt that many errors, and wrong notions, as to blood in the urine, have prevailed. In some the sensible characters so closely resemble those of bloody urine, that reliance upon them alone must lead to error.

Medical Times, July 21, 1849, *p.* 51.

53.—*On Suppression of Urine.*—By Dr. J. C. Hall.—[Dr. Hall speaks first of partial, and then of complete suppression.]

1st. *Cases in which there is a partial suppression of urine.*—Such cases are very far from uncommon. A gentleman exposes himself to a cold atmosphere—for example, remains loitering about in his garden, on a cold April day; in the evening he complains of having "taken cold," and creeps to the fire; he has an attack of shivering; he retires to bed; his skin becomes hot and dry; the urine is small in quantity, and of a dark colour. This attack often continues for some days, after which, under appropriate treatment, the urine gradually becomes secreted in larger quantities, and the patient recovers his usual health, the improvement commencing with the increased secretion of urine. This I have observed so often that no doubt remains on my mind as to its correctness, and its bearing on the treatment of disease is highly important.

The recent experiments of Dr. Golding Bird throw additional light upon this subject; he shows that the kidneys remove from the blood all excess of water, together with the metamorphosed products of effete tissues, or mal-assimilated food; that these organs are in reality, the "depurators" of the system, and the teachers of

the numberless chemical changes which are constantly taking place both in the healthy and diseased animal fabrics. He has come to the conclusion that from 600 to 700 grains of solid materials are daily separated by the kidneys of a person in a state of health. He has also constructed a very important table, by which, in a moment, the relation between the specific gravity of the urine and the amount of solids contained in an ounce of it may be ascertained. He says, "A glance at this table presents us with a mode of recollecting the quantity of solids existing in urine of different specific gravities, when the table is not at hand for reference—a piece of short memory of no small service in practice. Thus, if the specific gravity of any specimen of urine be expressed in four figures, the last two will indicate the quantity of the solids in a fluid ounce of the urine within an error of little more than a grain, when the density does not exceed 1.030; above that number, the error is a little greater. To illustrate this, let us suppose we have called upon a patient, the integrity of the depurating functions of whose kidneys we are anxious to learn. The quantity of urine excreted, we will suppose, in twenty-four hours, amounts to three pints, or sixty ounces, and the density of the mixed specimens passed in the time alluded to is 1.020; now, we have merely to multiply the number of ounces of the urine by the last two figures of the specific gravity to learn the quantity excreted, or $60 \times 20 = 1200$ grains of solids. If the table were at hand, the calculation would then be more rigid, for we should then multiply 60 by 20.79 instead of 20; the product, 1247 grains, shows, that by the former mode an error of forty-seven grains has been committed, an amount not sufficient to interfere materially with drawing our inductions at the bed-side, and, of course, capable of immediate correction by referring to the table at our leisure." Dr. Bird also draws attention to the fact, so well known to all whose eyes have wandered over the Hippocratic writings, that there is "an obvious relation between the termination of some phases of disease and a crisis by urine." Simon says, that some physicians have pointed out, that for the most part they have observed a lateritious deposit in the urine at the period when patients begin to recover from typhus; hence the very important practical questions, at what period do these deposits take place? and is there any reason for regarding them as harbingers of a happy result? Willis and Schöenlein consider that they are to be looked upon as favourable; on the other hand, Becquerel considers this deposit as affording no evidence of the result of the disease; in some of the fatal cases under his observation, a deposit was observed at various stages of the disease, and Andral has made the same remark.

This opinion does not coincide with that of Dr. Golding Bird; nor in the comparatively limited sphere of my own experience has it found materials for confirmation. In the days of old, men observed for themselves, and truthfully, carefully, and patiently recorded the result of what they had seen. Dr. Bird is far from despising this empirical knowledge of our forefathers; but rather attaching a

proper degree of importance to their bed-side lore, he has been enabled by the powers of modern science, to prove that what was said by the old physicians, with regard to a crisis by the urine, is founded in fact. He gives the results of several cases of intermittent fever which were under his care in Guy's Hospital, and remarks: "If I can prove that, concomitantly with an enormous increase in the excretions of the kidneys, sudden improvement occurs in a patient,—which change for the better does not take place until this great change is observed,—I think it will be conceded that I shall produce sufficient evidence to show that the observations of our predecessors were correct, and that something like a critical excretion from the kidneys does take place,—at least in the diseases which have been sufficiently carefully studied in this point of view."

Another writer, whose opinions command the greatest respect, and whose field for observation has been almost unlimited, Dr. C. J. B. Williams, remarks, that "the excretions are defective in many idiopathic and symptomatic fevers," (Mr. Palmer detected urea in the serum contained in the ventricles of the brain, in a case of fatal apoplexy from granular kidneys and diseased heart;) "and it is when the secreting organs recover their power, and a diarrhœa occurs, or a copious discharge of highly-loaded urine, that a favourable change occurs." In future papers, I shall be able to prove also, that morbid matters, which, by altogether remaining in the system, produce alarming symptoms, and speedily end in death, in a slighter degree, make up the elements of disease; and the leaves of my case-book contain the record of cases evidently the result of defective excretion, although at one time we should have been inclined to regard them in another light. We have now to consider,

2ndly. *The treatment of those cases in which there is a total suppression of urine.*—It remains only to say, in conclusion, a word or two on the treatment of those singular cases to which Sir H. Halford first of all directed our attention,—in which, independent of acute disease—independent of any derangement he could discover in the structure of these organs—their secreting office was completely suspended. This constitutes the disease which Dr. Willis has named "anuria,"—said by him to terminate in coma in four or five days, and in death in a few days more. I venture to say that a fatal result, according to my experience, occurs much more quickly. In three out of the five fatal cases which came under the observation of Sir H. Halford, a very strong urinous smell was observed in the perspiration for twenty-four hours before death. The patients all died with "symptoms of oppression on the brain."

I have little to add to what has been said already with regard to treatment. Looking at those cases of cholera which have recovered, in which suppression of urine has continued some time, I am inclined to advise, at the first, that a full dose of calomel and colocynth, with a drop of croton oil, should be made into a pill and given to the patient; this to be followed by an aperient draught of infusion of senna, sulphate of magnesia, and cream of tartar. A large

stimulating glyster should also be exhibited, so that the bowels may be largely purged three or four times; cupping over the loins, bleeding, if the state of the pulse admit of it; warm baths; a large blister to the loins; and full doses of powdered cantharides,—a remedy which was very successful in the hands of Sir A. Cooper, and which in one case of total deprivation of urine that I have been consulted in, was most certainly followed by a re-secretion of urine, after all hope of success had been abandoned, and the recovery of the patient regarded as all but impossible.

This plan of treatment by powerful diuretics may, theoretically, appear incorrect, but, so far as we know at present, it is one that has been found, practically, the most successful.

Lancet, July 7, 1849, *p.* 5.

54.—*Production of Sugar in the Urine by Wounding the Brain.*—By M. Bernard.—On the 26th of March, 1849, M. Magendie announced to the French Academy a very important and entirely unexpected discovery made very recently by M. Bernard. M. B. has found that by wounding a certain part of the floor of the fourth ventricle, the composition of the urine becomes altered, and sugar makes its appearance in it.

The puncture is made by passing the instrument through the inferior orifice of the ventricle, and soon afterwards the urine of the animal (a rabbit), which before the operation is turbid, alkaline, and free from saccharine matter, becomes abundant, clear, and contains in solution a very large quantity of sugar, and resembles that of diabetes. In general, not more than an hour and a half or two hours are requisite for the complete production of this change in the characters of the urine. The blood also contains a large amount of sugar.

The experiments have hitherto been made upon sixteen rabbits; and by varying them, M. Bernard has found that the part of the fourth ventricle which must be wounded to produce this remarkable phenomenon of the appearance of sugar in the blood and urine was very limited, and corresponded to a space situated a little above the origin of the eighth pair of nerves.

These results which are so surprising from their novelty, cannot at present be in any way explained. They merely serve to show the remarkable influence which the nervous system exerts upon the functions of nutrition; and in this light they deserve the serious attention of chemists.

The experiments which have also brought to light some other interesting particulars, are being continued upon animals of different kinds, and placed under various circumstances. M. Bernard hopes soon to be able to lay the results before the Academy.—*Chem. Gaz.*, May 15, 1849.

Monthly Retrospect, July, 1849, *p.* 139.

SURGERY.

FRACTURES AND DISEASES OF BONE.

55.—ON THE SYMPTOMS OF FRACTURE OF THE BASE OF THE SKULL.

By John Adams, Esq., Surgeon to the London Hospital.

[In reply to the question "What are the pathognomonic signs of fractures of the base of the skull?", Mr. Adams observes:]

Hemorrhage from the ear is generally ranked as a symptom of fractured base: this may arise from a variety of sources—thus, it may depend on rupture of the membrana tympani; it may depend on rupture of the lateral sinus, or of the carotid artery, as when an extensive fracture traverses the canal lodging this vessel: in either of the two latter instances the hemorrhage would be so profuse as to leave no doubt as to the nature of the injury. But hemorrhage from the ear is, in my opinion, not pathognomonic of this accident: it is so commonly found in slight cases of injury to the head, and in cases which occur without any untoward symptoms, and many such cases are brought to this hospital, that I feel justified in discarding it as a sign wholly to be relied on. Now, if in a case of injured head, hemorrhage from the ear has taken place, and the injury has been accompanied or succeeded by paralysis of the portio dura, then I should be disposed to regard these signs collectively as indicative of a fractured base; but separately and singly, although it is an important indication, I do not rely upon it as conclusive of the existence of such an accident. Take paralysis of a cerebral nerve: I believe that in many cases permanent or long continued paralysis of parts supplied by any nerve passing out of the base of the skull will afford presumptive, though not positive, evidence of the existence of fractured base: although you must understand that this may result from injury to the brain or root of the nerve irrespective of fracture.

Although hemorrhage from the ear and paralysis of nerves passing out of the base of the skull are presumptive signs of fractured base, yet they are not positive signs of this accident; and, indeed, I know only of one symptom which is strictly pathognomonic of the injury, and that is a free discharge of serum from the ear after a severe injury of the skull. This symptom, however,

comes on, not immediately, but on the second or third day after the accident. I remember a case some years ago, of a man in whom a serous fluid escaped from the ear in such quantities as to soak through the pillow, and to require repeated changes of linen. A similar case is recorded in the tenth volume of the Medical Gazette, where the patient lost between four and five ounces of a similar fluid in one night; both these cases did well. In the Mémoires de la Société de Chirurgie de Paris, there is an article on this subject by M. Robert, who, from numerous dissections, has distinctly traced this phenomenon to a fracture extending across the petrous portion of the temporal bone, and a direct opening from this into the arachnoid cavity.

Medical Gazette, June 1, 1849, *p.* 926.

56.—*On the Mechanical Treatment of Rickets.*—By E. F. Lonsdale, Esq., Assistant Surgeon to the Orthopædic Hospital.—The curvatures depending upon rickets are more difficult to treat mechanically, than those arising from simple weakness, owing to the deformities being so much more severe, as well as taking such a variety of shapes. In a great number of cases the knock-knee and outward curvature of the tibia are combined; two kinds of splints, then, have to be employed—namely, the long splints outside to act upon the knees, and the short ones inside to act upon the tibia. The short ones are to be applied first, and then the long ones on the outside, making the knee strap pass round the two splints, and the ankle one the same, the middle strap round the leg acting upon the short splint only: pressure upon the joints and bones continued in this manner for some time, will both straighten the knees and the bones of the leg. The inner splint may be necessary in slight cases, where there is only the indentation just above the inner ankle, to prevent the ankle strap that fixes the lower end of the long splint from slipping into the hollow so formed, and tending to increase the curvature. It need not be very wide when used for this purpose.

Little can be done for the *anterior* curvature of the tibia, for it is not only difficult to apply the pressure, but when applied it cannot be borne to an extent sufficient to act upon the deformity, owing to the skin becoming soon irritated on the sharp spine of the bone: as a general rule, however, this anterior curve diminishes as the bone elongates by growth, the projection occupying a higher position nearer to the centre of the tibia.

The mechanical treatment required for the severer forms of rachitic genu valga is the same as that recommended for the severe kind of common knock-knee—namely, the splint, with a hinge to allow it being fitted closely to the shape of the joint, the extension outwards being made by the male and female screw; the important point to attend to being to see that the knee lies flat in the splint, and that it is firmly fixed in position with a broad strap and thick pad. After the bones have been brought into the straight

position, they require to be kept so, by means of artificial support for some months, when the patient begins to walk. This support is gained by employing irons attached to the boot below, and to a pelvic hoop above, which connects the two together. Each iron has three joints, one at the hip and ankle, which are free, and one at the knee which locks with a "catch," and admits of being used at pleasure, so allowing of the joint being bent, when it may be thought proper to do so; which, however, should not be done till some time after the patient has commenced walking: the object being to let all the ligaments and muscles around the joint become accustomed to their new position, into which they have been brought by the deformity being removed. The irons should be made as light as may be consistent with strength, more particularly in young children, where the muscular system generally is weak. When the patient discontinues the use of irons, he should do so by degrees, by leaving them off for a short time every day, and so increasing the interval; the guide being, the existence or not of any inclination in the joint to fall inwards; should this exist, they must be worn constantly for some time longer.

Severe outward curvature of the tibia, when depending upon rickets, is generally accompanied with outward curvature of the femur also, throwing the knees widely apart. It is a very difficult deformity to treat even in young children, owing to the inability to gain sufficient purchase on the thigh to act with any mechanical power on the curvature of the femur itself; for to straighten a bone by means of a splint and straps, a fulcrum must be gained at both ends, while the pressure is made to tell upon the centre and most curved part; and this cannot be done on the thigh, from the peculiar attachment of the head of the bone to the pelvis. The common splint, however, is the best to be employed, and should be made long enough to extend up the thigh as high as practicable: the pad of the knee must be very thick to throw the splint off from the thigh, so that the straps may have a purchase on the bone, and act upon it, if it be inclined to yield; whereas if the pads were thick on the thigh, all the straps would do would be to press the muscles against the femur, instead of tending to draw the bone towards the splint. But, as before stated, it is very difficult to act upon the thigh bone at all in these cases. Much can be done in removing the curve in the bones of the leg, which will also assist in removing that of the bone of the thigh, by bringing the weight of the body to bear more perpendicularly upon the whole limb.

Another difficulty in these cases of severe curvature of the thigh bones, when met with after the bones have become hardened, is, that any straightening that may be produced in the limb is done at the expense of the knee joint, provided the bone itself will not yield; for the articular surfaces of the bones—namely, the tibia and fibula—remain in contact notwithstanding the great degree of curvature that may exist in them. It is different in the cases of knock-knees, where the inner condyle and the inner portion of the head of the tibia may become separated to a great extent, the sep-

aration being distinctly felt by placing the finger between the two. This must be borne in mind; and it is better to leave the curvature in its original condition, rather than strain the joint to an extent to produce a weakness which the patient may never altogether get rid of. A good rule to follow in these cases of curved bones is to ascertain if the curvature is increasing; for, if so, it necessarily implies that the bone is still soft enough to yield, and if in one direction, there is no reason why it should not yield in the other, though the weight of the body may cause it to do so more than the pressure produced by the straps and splints; still, by perseverance in keeping the patient off his feet as much as possible, much may be gained.

Medical Gazette, Aug. 10, 1849, *p.* 232.

57.—*On the Treatment of Fractures of the Tibia.*—By W. Allison, Esq., East-Retford.—[Mr. Allison states that he can strongly recommend the following plan of treating fractures of the tibia, it being that which he always adopts:]

On all occasions, much care is requisite in preparing the bed before the patient is placed upon it. Having procured a board nine or ten inches broad, and long enough to reach from the middle of the thigh (or if the leg requires raising, from the ham,) to some distance beyond the frame of the bed-stock, it has been pushed under the bed, and the feathers have been pushed beyond each side of the board, until the bed was no thicker than one or two inches, in order that the ends of pieces of strong tape might be sewn firmly to the bed, along the line over which the rollers are to be placed, at such distances from each other as to admit of their being tied, from the outer side of the rollers, a little below the knee, over the centre of the leg, and just above the ankle. In sewing the ends of the tapes, a long needle has been pushed from underneath the bed, through it and through the tape upon it. On some occasions tapes have been fixed for tying above the knee also (over the thigh), especially when there has been much spasm of the muscles; when the patient has had disturbed sleep, or has been naturally irritable, and very restless.

During the time occupied in preparing the bed, two sheets have been rolled up tightly, long enough to reach from above the knee to so far beyond the end of the foot, as to admit of being sewed together, including a half brick, folded in calico, at the end of the heel for the purpose of afterwards admitting a pile of covered bricks for a foot support. The rolled sheets have been rolled from each side of a piece of calico, about a yard broad, and the same length as the rolls, towards the centre of it, until they would just include a narrow pillow, placed upon padding above and below where the calf of the leg is to rest, so as *to avoid pressure on the heel*, and after the leg has been placed upon the pillow, having the feathers pushed from the centre, and defended by oiled silk, and three or four thicknesses

of calico, folded lint has been placed upon the tibia, both above and below the fractured part, so as to keep off pressure from a splint placed along the bone, upon which the tapes are to be tied. Sometimes two tapes have been tied round the pillow and leg, the one above, and the other below, the calf, before tying the rollers; the rollers then have been rolled completely up to the pillow in which the leg was enveloped, and have been tied there with the three tapes on each side, over the splint.

If the patient was a poor man, having a bed only, (without a mattress), supported by cords, the board should not reach higher under the bed than the ham, because the hips will sink down into a hollow, and the board, with the thigh, will form a double inclined plane. The contrivances, of course, will be varied according to circumstances, whether any side splints be needful within the rolls, or any elevation of the foot above the knee, when the leg is much swollen.

In cases of compound fracture, the limb has been placed in the most convenient position for exposing the wound, whether the patient has been upon his back, with the leg straight, upon his side, with the knee bent, or half way between the two positions. If on the side, the front roll, of course, has reached from the knee to the instep only.

The wound has been covered with doubled lint, a little bloody; over it a larger piece of doubled lint, soaked with the compound tincture of benzoin, and over that a piece still larger, soaked, so as to seal up the wound air-tight. If the leg be easy, and the lint free from any bad smell, the lint may remain ten or eleven days undisturbed; if uneasiness or offensive effluvium indicates the necessity of removing the lint sooner, the wound will show whether a poultice or any other application be desirable. The foregoing was the practice adopted in the Sheffield General Infirmary upwards of a quarter of a century ago.

Provincial Medical and Surgical Journal, July 11, 1849, *p.* 378.

DISEASES OF JOINT, &c.

58.—ON ARTHROPATHIA OF THE SHOULDER-JOINT.

By M. Velpeau.

M. Velpeau designates the affections of the joints under the general term arthropathia; and in the present paper, a peculiar form of disease affecting the shoulder-joint is illustrated by several examples. Every surgeon meets with cases which are characterised by atrophy of the muscles of the shoulder, especially the deltoid; and numerous autopsies have assured M. Velpeau that the osseous portions of the joint participate in the atrophy. The deltoid may be reduced to almost a membranous condition, scarcely

exhibiting any muscular fibres; and not only is the glenoid cavity diminished in size and depth, but the head of the humerus is lessened, and even its body diminished in volume and length. The cartilages disappear early, not as in ordinary caries, but sometimes by a sort of atrophy, and at others by undergoing osseous transformation.

The disease may commence as ordinary arthritis, but is soon found to differ from this by the slowness of its progress (M. Velpeau has known cases occupy more than twelve years before complete), and the accompanying atrophy. The original part affected seems to be the soft parts, especially the deltoid; and the constancy with which this is the case has led M. Velpeau to conjecture, that an affection of the circumflex nerve might be the original one. There are three distinct stages: the period of painful affection of the brachial plexus, or only of the circumflex, which, with the accompanying swelling, has led to the disease being mistaken for rheumatism; the muscular atrophy; and the changes in the bones. In two of the cases cited there was observed a semi-luxation downwards, of easy temporary reduction. This luxation may be due to the elongation of the muscular fibres allowing the escape of the head. In treating these cases, three periods have to be observed. During the early and inflammatory period, repeated leechings, or, better still, small cuppings, friction with mercurial and belladonna ointment, and the application of emollient and resolvent cataplasms, are indicated. This period passed, large blisters, moxas, or the cautery before and behind the joints, baths, with local stimulants and tonic douches, either of plain, or sulphureous, or aromatic waters, are demanded. In the third stage, when the bony parts are affected, the derivative and antiphlogistic treatment is to be laid aside, and frictions, especially with the essential oil of turpentine, resorted to. They rouse the dormant vitality, and produce a cure in many cases apparently hopeless. The sagacity of the surgeon will decide between the two risks of causing anchylosis by leaving the arm too long unmoved, and the re-excitement of inflammation by moving it two soon. Of the two dangers, the first is the greatest; and in order to prevent it, we must not fear exciting a little pain. Cases are detailed in which even subluxation had been produced; and yet treatment has soon restored the plumpness and full use of the arm.—*Bulletin de Thérapeutique*, tom. xxxvi, pp. 64-73.

British and Foreign Medico-Chirurgical Review, Oct. 1849, *p.* 559.

59.—*On "Bow-Legs."* – By E. F. Lonsdale, Esq., Assistant-Surgeon to the Orthopædic Hospital.—[This affection, outward curvature of the tibia and fibula, does not necessarily arise from any constitutional taint; it may originate simply from the bones being too weak to support the weight of the body. If there be general debility, or if the child has been badly nourished, and exposed

to impure air, attention to these circumstances should of course be our first object. When, however, the case has not originated in these conditions, or, under any circumstances, if the affection is severe, we should use mechanical means of relief. In cases of bow-legs, Mr. Lonsdale observes, though some children do certainly "grow out" of the deformity, the great majority will get worse, if left unchecked. As to the mechanical means to be adopted, Mr. Lonsdale says:]

For the common slight form of curvature, all that is required is, two calico bandages, two small wooden splints, and six webbing straps with buckles—three for each leg, two broad ones, and one narrow. The splints should be long enough to extend from the condyle of the femur down to the edge of the sole of the foot; they are to be padded at each end with a pad large and thick enough to cover the inside of the knee and the inner malleolus. If the pad be made in one piece, the two ends should be thicker than the centre. The splints should be quite as wide as the thickness of the leg—if anything a little wider: the object of this being to remove the pressure from the fore and back part of the limb, and making the straps press upon the outer side principally. They are applied as follows:—Bandage the leg evenly, from the toes upwards to a little above the knee; the use of this being to equalise the circulation and guard the skin from the irritation and friction of the straps and pads. The splint is then to be placed on the inside of the leg, so that the upper pad may rest on the inside of the knee and the lower one on the inner ankle: care should be taken not to allow the splint to fall forwards, or otherwise the lower end of it will press against the instep, and interfere with the child's walking, independently of preventing the pressure acting in the most favourable manner on the curve of the bone: the lower pad should also be thick enough to throw the end of the splint away from the foot. The knee and ankle straps should be attached to the splint in a position to correspond to these two joints when fastened, while the centre one should be left moveable to allow of its position being altered at pleasure, to accommodate itself to the most curved part of the bone opposite to which it should be placed. When the straps are tightened (and they should only be slightly so at first), the centre one should make the most pressure; care being taken to avoid pain and irritation on the skin, as well as swelling of the foot, which will occur if the ankle strap be too tight. If the inner malleolus be very prominent, and the skin very liable to become irritated by slight pressure, the lower end of the splint may be hollowed out opposite to the ankle; as a general rule, however, if the pad be made soft and thick, this is not required. The child should wear shoes, and not boots; the latter give no real support to the ankle joint, and only cause irritation to the skin when the straps are tightened on the pad that lies upon the boot: the former leaves the lower part of the leg quite free, and allows the ankle strap to be adapted to the lower end of the curve with more precision and effect than it would be if made to tell upon the hard leather of the boot.

The rules to be observed during the time the splints are being worn are few and simple:—To keep up sufficient pressure on the bone by gradually tightening the straps, more particularly the one or the two (if more than one are employed) that act upon the centre or most curved part of the bone. Never let the child stand without the splints; when it is necessary to take them off to wash the child, let it lie down until they are reapplied. To guard against irritation of the skin by relieving the pressure if the child complains of pain. To attend to the general health of the child by giving strengthening and other medicines if required, and procuring change of air if practicable. Not to leave the splints off at once when the bones have become sufficiently straightened; to do so by degrees, beginning with an hour or two a day, and increasing the time every second or third day; the object of this is to avoid throwing the weight of the body suddenly on the ankle and knee-joints, by doing which an unequal strain might be made upon them, and a weakness produced, and a tendency to reoccasion the curvature, owing to the bones not being kept in the perpendicular position. There is one other point not mentioned, with regard to the thickness of the pad at the knees; if there be any tendency to knock-knees they should be made very thin, otherwise they only prevent the approximation of the thigh bones, and throw the feet outwards, and bring the strain upon the knee-joints.

The length of time the splints will require to be worn must depend upon the period during which the deformity has existed, its degree of severity, and the age of the patient; as a general rule, the time will vary from six to twelve months; of course this being influenced by the above circumstances, as to whether it should be longer or shorter.*

Medical Gazette, July 6, 1849, *p.* 14.

60.—*On Genu Valgum, or Knock-Knees.*—By E. F. Lonsdale, Esq., Assistant-Surgeon to the Orthopædic Hospital.—[This deformity, Mr. Lonsdale observes, usually commences when the child is first put upon his legs, its immediate cause being mechanical, though the remote one may be constitutional. The treatment for slight cases consists in change of air, strengthening medicines and nutritious diet; while in severer forms of the affection, or if even slight deformity continues to increase, mechanical support should be adopted. Mr. Lonsdale thus describes the method which he recommends:]

The simplest treatment, and at the same time a very effectual one, is that employed at the Orthopædic Hospital: it consists of two long splints, extending from the hollow of the loins down to below the outer ankle, to nearly the ground. The upper end of the splints are notched with a slit to pass a long webbing strap through:

[* In these cases, a useful medicine is the phosphate of lime, ten or fifteen grains once or twice a day, mixed with the food of the child.—Ed.]

the object of this strap is to connect the upper end of the two splints together, to keep them from falling too much forwards. This back strap should be so attached to the splint that it may lie in the hollow of the loins, just above the projection of the buttock: if it come on this latter part, it only displaces the splint and pulls it upwards, removing the lower end from the ankle; and if the strap be attached too high up, owing to the splint being too long, it will not press against the child's back at all, and it will not steady the splints, besides causing an unnecessary projection beneath the child's clothes. Having procured splints of the proper length and width, (the latter being the width of the child's leg about the knee), they must be evenly padded, by making a long cushion of wadding or loose soft flock, and covered with calico and wash leather; (I often use dark-coloured leather in private practice,—it looks neater, and keeps cleaner). It is of importance to attend to the thickness of the lower end of the pad, which should be sufficient to throw the splint well off the outer ankle, to prevent the end of it pressing upon the instep, and so interfering with the child's walking. The pad should not come quite to the end of the splint: it must be fastened to the splint, either by turning it over the edge of it and running it across, or by piercing it through with small loops. If the pad and splint be not fastened together, neither the one nor the other will keep its place. If the common deal splints are used, the lower end is notched, to allow of the ankle strap (which must be longer and narrower than the one for the knee) being once twisted round it before it is passed round the ankle, or it may be tied and fastened through with a loop. By keeping the ankle strap fixed to the splint, the two are preserved in their proper position. Having placed the splints, then, on the outside of either leg, seeing that the upper ends are resting against the trochanters of the thigh bones, and the lower on the outer ankles, they are to be fixed in this position by buckling the upper strap across the loins, and the lower ones round the ankles. The next point is to confine the knees, and this is done by applying a broad webbing strap round the joint, taking care that it embraces the head of the tibia as well as the condyle of the femur. It is then to be buckled sufficiently tight to draw the knee outwards towards the splint. In slight cases the joints can be straightened at once by these means; in the severer cases it must be done more gradually, by tightening the straps a little more every second or third day.

By adopting the above treatment till the knees keep their proper position, when the splints are taken off, all cases of slight knock-knees may be cured in from three to six months. The severer cases occupy a longer time, and require other treatment, which I shall refer to in my next paper. One important rule to observe in the employment of the splints is, *never to let the child stand without them before the legs are straight*, and to keep the splints always on the outside,—not to let them fall forwards, when all purchase on the knee-joints would be lost. The legs must of course be evenly bandaged before the straps and splints are applied, to

prevent the feet swelling, and the skin being irritated. When the child is washed, and the bandages changed, it should always be made to lie down, to avoid any strain on the ligaments of the knee; for more mischief might be done by so doing, than could be recovered from for some time, and the treatment be thereby unnecessarily prolonged.

Medical Gazette, July 6, 1849, *p.* 18.

[In severe cases of knock-knees, it is difficult to keep these straight splints in position, and Mr. Lonsdale in such cases recommends the splint to be made with a joint at the knee. He says:]

I have employed the following kind of splint, made on the above principle, which enables the patient to move about, at the same time that the splint is kept in action, and in close apposition to the limb. Two narrow splints, about an inch an a half wide and a third of an inch thick, and long enough to extend from just above the trochanter down to the outer edge of the foot, are jointed at the point opposite the knee. To the two pieces thus connected a male and female screw are attached, so keeping the angle of the splint fixed in any position. The whole splint is evenly, but not thickly, padded with wadding, and covered with wash-leather. The splint is fixed to the leg in the following manner:—A stocking is made of wash-leather, to lace in front, and to extend from the instep up to the groin. To this stocking the splint is to be attached throughout its whole length, so that when it is applied evenly to the outside of the limb, and the stocking laced closely up, it cannot shift its place. Should it, however, be inclined to do so, owing to the severity of the knock-knees, this can be guarded against, by having a small slide made of thin iron to fit into a box in the boot or shoe, just in front of the heel, with a hinge to allow of motion at the ankle-joint. The upper ends of the two splints are also more firmly fixed, by having a broad strap or belt to pass round the pelvis and loins. The splints being thus firmly applied, the screw on the outside of each knee is gradually turned, to keep up traction on the joints, sufficient to draw them outwards more and more every second and third day. If the knee part of the leather stocking do not fit very close, an additional webbing strap must be passed round the joint and splint; otherwise, when the screws are turned, the splint will move without acting on the knee. A splint made and applied in the above manner looks exceedingly neat, and acts very efficiently, the child being able to walk about without any fear of the apparatus being displaced, at the same time that the knee-joints are kept in position. The small piece of iron fitting into the boot is a very efficient means of preventing the lower end of the splint from slipping forwards on to the instep.

Another very useful apparatus employed at the hospital by Mr. Tamplin, and adapted for the severest kinds of genu valgum, is the following: it consists of a kind of trough, having a back and outside piece, jointed opposite to the knee, and moved at any angle by

the common male and female screw. The splint is well padded, and has two sets of straps, so arranged that they fix the limb to the outer and back portions of it, by having one strap passing through the outer and another through the back part of the splint alternately, from the ankle up to the groin. The points to attend to in using it are—to fit the splint closely to the shape of the limb, by screwing it to an angle to correspond with the angle of the knee; to roll the limb evenly from the toes up to the groin; to see that the pad lies smooth and even; to place the limb quite flat in the splint, so that it may not roll on its outer side (when all action on the joint would be lost); to tighten the straps, beginning with the ankle one, making the pressure less on the thigh than leg. A very broad strap, with a separate thick pad, will be required for the knee, to be placed on the inside; this strap should be tightened sufficiently to keep the splint and knee in close contact, without producing pain. The limb being firmly fixed to the splint, the screw on the outside is to be gradually turned every second or third day, till the joint is straightened, which is ascertained by the leg and foot being brought into a line with the thigh and hip. This is a guide, provided the leg is lying flat in the splint. If it be rolled over on its outer side, it will look straighter than it really is. The splint I have just described is only used for the severest cases of knock-knees, when the patient is obliged to keep the horizontal position during the treatment.

In the most severe form of knock-knees it may be necessary to divide the tendon of the biceps flexor cruris. In cases of long standing, when this muscle has become so much shortened as to be a cause of preventing the straightening of the joint, mechanical means only may not be sufficient to overcome the deformity. A distinction must be made, however, in examining the joint, between the voluntary contraction of the muscle by the patient, and the permanent contraction which is found when the attempt is passively made to move the joint.

Medical Gazette, July 27, 1849, *p.* 145.

61.—*On the Treatment of Club-foot.*—By E. F. Lonsdale, Esq., —[In the treatment of some cases of club-foot, the use of mechanical means alone will be sufficient. As a general rule, however, the division of tendons should also be had recourse to. Upon this subject Mr. Lonsdale observes:]

The division of tendons, though apparently a simple operation, requires great nicety in its performance, and in many cases a good deal of experience, both in finding the precise situation of the tendon, as well as in knowing when it is divided. In this latter part the surgeon often must depend upon him who assists in the operation to know for certain whether the tendon has been divided at all, or if partially instead of completely: the tendon to which these remarks mostly refer is that of the posterior tibial muscle. In

infants and adults very often it is difficult for the operator always to know when he has cut it through; he trusts then solely to the assistant, who should be able to tell by the amount of yielding of the foot, as well as by feeling the substance of the tendon divided at the same time, whether such be the case or not; and if any doubt arise, either in his or in the operator's mind, the knife should be again passed beneath the tendon, and attempts made again to divide it.

The tendon most easily divided is the tendo-achillis: its position and size, as well as its separation from the deeper parts, offer great facility in ascertaining its precise boundaries, as well as for the passage of the knife beneath it. Some precautions, however, are here necessary to avoid untoward circumstances, the two principal of which are, transfixing the skin, either in passing the knife inwards, or after the tendon has been divided; the other precaution to take is, to avoid the posterior tibial artery, which, however, is more necessary in infants than adults; in very many cases of infantile varus the tendo-achillis becomes so much displaced inwards that it lies almost over the artery, and nearly in contact with it. If the point of the knife be passed too deeply before it is made to turn beneath the tendon the vessel might be punctured. This accident is more likely to happen in very fat children, and in very thin ones, owing to the great nicety then required in calculating the precise depth at which the tendon lies.

In some of the violent spasmodic cases of contraction of the muscles of the calf, great care is required to prevent the knife dividing the skin, when the ends of the tendon yield so suddenly, and bring the edge of the knife against its inner surface before the surgeon might be aware of it. Much depends also upon the assistant in these spasmodic cases to watch closely when the division of the tendon takes place, and to immediately relax the foot at the same time that the operator turns the edge of the knife downwards, before withdrawing it.

The posterior tibial tendon is certainly the most difficult to divide, both from the absence (as a general rule) of any definite shape or size to serve as a guide as to its *precise* situation. In very fat infants it is next to impossible to feel the tendon itself through the skin; and to feel the only sure guide to it, namely, the inner edge of the tibia. Much tact and experience is required in these cases, in the assistant as well as in the operator.

The posterior tibial artery is sometimes wounded and divided completely, and this is not to be wondered at, when the close position of the vessel to the tendon is considered, for there is no certain guide to the thickness or to the depth at which the tendon may be placed: I never have seen any evil consequences myself arise from it, provided it be completely divided across; the vessel then retracts, and closes here as in other situations, and all fear of aneurism is avoided. When the artery has been divided, graduated compresses should be placed over it, and the limb be evenly bandaged from the toes upwards, and should not be looked at for a fortnight or three weeks.

The division of tendons, though so important and nice an operation, is certainly one of the most harmless in surgery, when done with care and attention: for out of the many cases that I have seen operated on, and have operated on myself, together amounting to the division of some hundreds of tendons, I do not remember a single case during the last five years where the wound has not healed by the first intention, without the slightest approach to inflammation, or any untoward circumstance. It is this fact that ensures a more certain cure now than when the operation was first introduced; for the harmlessness of the division of tendons necessarily admits of more boldness in this branch of surgery, by allowing the surgeon to divide with impunity all those parts, whether tendons or bands of fascia, which exist as obstacles to the perfection of the cure. The grand reason, no doubt, of the safety of the operation, as well as of the certainty of the union of the two ends of the tendon, is owing to the division being so completely subcutaneous, there being simply the puncture in the skin produced by the narrow blade of the knife, and which heals in the first four and twenty hours. Reparation then goes on without the possibility of any interruption, provided the foot be kept at rest, and its natural warmth preserved.

Two methods are employed for the division of tendons; the one, that of passing the knife beneath the tendon, and then cutting upwards towards the skin; the other, that of passing the knife first between the skin and the tendon, and then cutting downwards, or from the skin. My own impression is that the former is decidedly a more preferable mode of operating than the latter.

Rest, amounting to the prevention of all movement between the two ends of the tendon, must be preserved for the first eight and forty hours. This is gained by placing the limb in the horizontal position, and by fixing the joint in such a position that the divided portions of tendon may be in as close contact as possible, to secure the first stage of the union commencing without interruption.

Warmth is another essential point to attend to, more particularly in cases of division of the tendon in paralysed limbs. The whole limb should be kept constantly covered either in a flannel bandage, or be wrapped up in flannel.

[It must be remembered, however, that the division of tendons is only the preliminary step to the use of those mechanical means, without which the cure cannot be effected. Mr. Lonsdale remarks, that whatever be the kind of machine or instrument used, the principal points to be attended to are,—to begin the extension at a sufficient and yet not too great interval after the division of the tendon,—to avoid unequal pressure,—to make the extension very gradual and regular, and so to conduct it that it shall not produce pain,—and to insist upon it that the patient himself shall not interfere with the apparatus except it produces pain.]

Medical Gazette, Oct. 5, 1849, p. 575.

AMPUTATIONS, &c.

62.—CASE OF RESECTION OF THE HEAD OF THE FEMUR.

By W. Fergusson, Esq., Surgeon to King's College Hospital.

[This was a case of morbus coxarius in its last stage, the patient being a boy of ten years old. We are told that the patient having been put under the influence of chloroform,]

Mr. Fergusson carefully examined the parts with his finger, and then commenced the operation in the following manner:—An incision, about four inches in length, was made over the head of the bone, and carried below the great trochanter; the soft tissues were dissected from the sides of the bone, and the knife cautiously carried behind its neck, separating, as much as possible, the soft parts; the limb was then carried inwards by an assistant, and the section made by a small saw, below the trochanter major, and that process, together with the head of the femur, removed.

The cotyloid cavity was then examined, for the purpose of ascertaining if there was any disease to be removed, but it was discovered to be filled up with a mass of soft tissue which had been thrown out during the course of the malady. Several small vessels required a ligature; the wound was brought together, and the patient removed to bed. After the removal of the bone, the limb could be brought down straight with the other.

Mr. Fergusson, in the course of some observations, stated this to be one of those cases which surgeons, in London at least, denominated morbus coxarius in its last stage, and that the little boy, who had laboured for eighteen months under the disease, was sent into the hospital for his opinion as to the propriety of performing the operation of re-section of the head of the femur. When the patient was first brought into the house, it was evident that dislocation existed; there was a large open sore over the trochanter major, and a profuse discharge of matter coming from it, which had greatly debilitated him. In order to ascertain the precise condition of the bone, and also to give some relief to the patient, a counter-opening above the trochanter had been made, and the head of the femur exposed, lying on the dorsum ilii, and in the centre of a large abscess. The cartilage could be felt on the bone, but its structure was so softened, that it could easily be penetrated by means of a probe. This measure had given the child some relief, but little hopes of further improvement; Mr. Fergusson regarded the head of the thigh bone as a foreign substance, keeping up irritation; and as he could not ascertain that there was any disease within the pelvis, nor in any organ of the body, he, therefore, considered it best for the patient to give him the benefit of the operation.

He had performed it in the same manner as they had before seen;

he had removed the trochanter as well as the head of the bone, not because there was any disease within that process, but, because he had found from experience, that the wound healed more readily, and less irritation was afterwards caused, than when it was permitted to remain. They had seen that the limb could be straightened after the diseased part was taken away, and they would be able to keep it in a good position by the application of a splint.

With respect to the condition of the acetabulum, it was probable that in this instance disease had at one time been present; but in this, as in other cases, after dislocation had taken place, and the two surfaces of the joint were no longer in contact, a reparative process had followed, and the morbid action in the acetabulum had ceased.

There had been much discussion as to the propriety of this operation; it was a subject which admitted of, and ought to have, a fair and open discussion amongst surgeons; he looked upon the present case as one of great interest, and he hoped that it would turn out satisfactorily.

Medical Times, July 28, 1849, *p.* 73.

63.—*On a Modification of Syme's Operation.*—By Dr. R. J. Mackenzie, Assistant Surgeon to the Edinburgh Infirmary.—[While he believes that the incisions now recommended and practised by Mr. Syme, will, in the majority of cases, ensure a good and useful stump, Dr. Mackenzie yet says]

I am induced to direct attention to the following mode of operating, from the fact that, in some cases, it appears to me to possess advantages over the other operation; whilst, occasionally, cases may occur where the integuments of the heel may be saved to form a covering for the ends of the bones, only by the following incisions:—

On the right foot.—The body reclining on the right side, and the foot and ankle projecting beyond the table with their internal aspect upwards, the point of the knife is entered in the mesial line of the posterior aspect of the ankle, on a level with the articulation, carried downwards, obliquely across the *tendo achillis* towards the external border of the plantar aspect of the heel, along which it is continued in a semilunar direction. The incision is then curved across the sole of the foot, and terminates on the inner side of the tendon of the *tibialis anticus*, about an inch in front of the inner *malleolus*.

The second incision is carried across the outer aspect of the ankle in a semilunar direction, between the extremities of the first incisions, the convexity of the incision downwards, and passing half-an-inch below the external *malleolus*.

The flap is now dissected up, care being taken that the knife cuts close on the bones, so as to preserve the whole thickness of the soft parts. By holding the base of the flap between the fingers and thumb, as it is detached from the bones, all risk of wounding

the artery at this stage is avoided. The foot is then severed at the articulation, and the ends of the *tibia* and *fibula* cleared so as to allow of the application of the saw, by which a thin slice of the extremities of these bones is removed, as in Mr. Syme's operation.

This operation is very easily and rapidly performed, and, with a very moderate amount of care on the part of the operator, the artery runs no risk of being injured. It is in clearing the ends of the *tibia* and *fibula* for the application of the saw in this, as well as in Mr. Syme's operation, that the artery is, I believe, in the greatest danger of being wounded. It is avoided by cutting close on the bones, a point to be attended to during the whole dissection in either operation.

The operation on the left foot is the same, but, as the patient reclines on the left side, the order of incision is reversed, the knife being first entered at the point above indicated, in front of the inner *malleolus*.

M. Sedillot of Strasbourg has since recommended a similar method of performing the operation. The incisions made by M. S. scarcely differ at all from those I have recommended; but their order is reversed, the first incision being made on the fibular aspect of the foot, the articulation opened, and the flap cut from within outwards.

The cases in which, I think, this operation may with advantage be substituted for Mr. Syme's method, are,—1st, those in which the integuments on the outer side of the ankle are in an unsound state either from disease or injury; 2nd, in elderly persons, whose arteteries are not well suited to bear with impunity the stretching, which occasionally occurs in separating the integuments from around a projecting heel; lastly, it may, from the ease with which it is performed, be preferred by those who have not the frequent opportunities of operating on the dead body, which are requisite for enabling them to perform the other operation with facility.

Monthly Journal, August 1849, *p.* 951.

64.—*On the Question of Amputation in Traumatic Tetanus supervening upon Injury to a Limb.*—By Bransby B. Cooper, Esq., F.R.S. —[Speaking upon this question, Mr. Cooper first reminds us, that in cases of traumatic tetanus, the injury can only be regarded as its *exciting cause*, there being always a kind of *tetanic diathesis* pre-existing. He then proceeds to say:]

Baron Larrey, during the war in Egypt, amputated in several cases, after symptoms of tetanus had commenced, and this practice was attended with sufficient success to induce him to recommend it. Instead, however, of the general adoption of this plan, I should recommend that the condition of the wound should first be strictly examined, and also the state of the constitution of the patient; so that where there appears the least tendency to tetanus, prophylactic measures may be taken to ward off the attack. When, for ex-

ample, there seems reason to dread an attack of tetanus in consequence of a punctured wound, the latter ought to be converted into an incised one by laying it freely open to the same depth as the original puncture; and, should it then be discovered that a branch of a nerve had been punctured, or partially divided, it ought at once to be cut completely through, so as to remove the continued irritation that would arise from its being only wounded. It has been said that suppuration prevents the accession of tetanic symptoms, and that it even relieves them after they have set in. If this could be proved to be the case, it would of course be desirable to promote suppuration as a prophylactic means of treatment. My own experience leads me, however, to doubt the correctness of the hypothesis; for I have have witnessed three successive cases of tetanus in which the symptoms first appeared during the progress of suppuration, and in each of these cases the discharge of pus went on until the death of the patient. I cannot, therefore, approve of the plan adopted by some surgeons, of cauterising the wound, to induce suppuration. The following case is one in which I was induced to amputate a limb after tetanic symptoms had supervened. In this instance, I was the more disposed to have recourse to this practice, as the injury was so severe that there existed no probability of the the limb being restored to a state of usefulness.

William Marshall æt. 11, was admitted into Guy's hospital on April 14th, 1849. His left leg and foot had been very severely injured by a luggage train crushing it again some brickwork. There was a large wound on the inner side of the left tibia, extending upwards from the malleolus to the extent of two inches. The saphena major vein was exposed, the posterior tibial artery torn through, and the nerve laid bare. It was considered advisable by Mr. Hilton to endeavour to save the limb, and he therefore placed a ligature on the proximal extremity of the divided artery, brought the soft parts together by sutures and strapping, and applied an outer splint with a foot-piece. The limb was kept in a somewhat elevated position. As the lad was in a state of partial collapse, stimuli were administered; reaction was consequently produced soon after.

On the day after his admission, he had recovered from the shock: had slept during the night, and the limb was easy. On the 16th, the sutures were removed, and the wound was found to be in a sloughing condition. The sloughing continued for the next two days; a sanious discharge had also commenced, and the lad had become irritable and restless. He remained in nearly the same state until the 26th, when I first saw him. I at once saw reason to fear that there was a tetanic tendency, and I desired that I might receive notice the moment any muscular contractions or other tetanic symptoms were manifested. On the evening of the 26th it was observed that the injured limb was drawn up by spasm, and that there was a twitching of the muscles, and the patient was altogether very restless. On the 27th there were distinct symptoms of etanus; the corners of the mouth were drawn up, nose pinched, and there was general rigidity of the facial muscles. He was ex-

tremely restless and irritable. These symptoms all continued to increase during the morning, notwithstanding treatment, and I was accordingly sent for, as I had requested. As soon as I saw the boy, I determined upon immediate amputation, and this was performed below the knee by the circular incision. Five ligatures were required. Four hours after the operation the wound was dressed. There was no subsequent bleeding.

On the 28th all the tetanic symptoms had disappeared; the face of the patient was restored to its natural expression; his mouth could be opened freely, and he was less irritable. He continued to go on well for some days, so far as concerned his health, taking plenty of nourishment, but there was no indication of healing in the stump. On the 1st of May, he became again restless and irritable; there was slight redness of the left knee, and some swelling of the right foot, with tenderness upon pressure. No reparative action had been set up in the stump. All these symptoms continued to increase, the swelling of the right foot extending up the leg to the abdomen; the breathing also was hurried, and there was slight cough. The stump still shewed no disposition to heal, and poured out an unhealthy sanious discharge. The symptoms continued unchanged, the patient daily became weaker, until the 11th of May when he died. *Medical Gazette, Sep* 14, 1849, *p.* 443.

65.—*Case of Re-union of Fingers after complete Separation.*—By J. Denny, Esq., Resident Surgeon to the Stoke Newington Dispensary.—A labouring man applied to me to dress the thumb and forefinger of the left hand, having, as he stated, met with an accident, whilst cutting or chopping a handful of grass with a sickle. Upon examination I found he had, by a clean incision, cut out of the thumb a triangular-shaped piece, the incision extending from the end down the centre of the nail, nearly to the root, then outwards towards the forefinger. The piece thus disunited consisted of the portion of nail described, integument, muscle, and a minute portion of bone. From the finger he had nearly sliced off a piece of muscle and integument on the side next to the thumb. I sent him back the distance of two miles, to search amongst the grass for the dismembered portions, which he succeeded in finding, and which, upon his return, I carefully washed with warm water, and adjusted in exact apposition to the surfaces from whence they were cut. I freely applied collodion, so as effectually to exclude the atmosphere, and prevent any further hemorrhage, and with narrow pieces of strapping held them firmly in the position in which I had placed them. The result has been the perfect re-union of both pieces, leaving little or no cicatrix.

I should mention that the period that elapsed from the occurrence of the accident to the replacing of the parts was four hours; also that the pain, which was very acute, from the exposure of the cut surfaces to the atmosphere, ceased immediately that the parts were replaced; and the man experienced little or no pain afterwards.

Lancet, Sept. 15, 1849, *p.* 295

ORGANS OF CIRCULATION.

66.—CASE OF ANEURISM OF THE AXILLARY ARTERY.

By Henry Hancock, Esq., Surgeon to the Charing Cross Hospital

[The patient was a bargeman, thirty-four years of age, and on the 19th August, 1848, when he came under Mr. Hancock's care, he stated that he had first perceived the swelling about six months previously.]

At the time of admission, a large pulsating tumour occupied the whole of the axilla, and extended under the pectoral muscles, which were pushed forwards, the clavicle being considerably raised. The tumour also extended down the upper two-thirds of the arm. He could not bring his elbow to his side; his hand was swollen and numbed, and there was great loss of power and intolerance of motion in the entire limb. The tumour, which diminished on pressure, again augmented when the pressure was removed: it also diminished when the subclavian artery was compressed; there was evident bruit de soufflet, and the aneurismal thrill could easily be detected by the touch, whilst the pulsation of the tumour was evident to the eye.

He was ordered to be bled to fourteen ounces, to take two grains of alum thrice daily, with a grain of the muriate of morphia at night.

He continued this medicine until the 26th, when I placed a ligature on the subclavian artery, at the first rib, immediately external to the scalenus anticus muscle. This was done without much difficulty, and with very little disturbance to the surrounding parts; the only difficulty experienced was from the depth of the artery, which I could but just reach with the point of my index finger; and for the same reason it was found necessary to diminish the curve of the silver aneurism-needle very considerably before I could insert it beneath the vessel. The ligature having been tied, pulsation could no longer be felt in the tumour; the wound was brought together by sutures and strappings, and the patient put to bed with the arm elevated, and enveloped in warm flannels, by which the temperature of the limb was well kept up during the night and next day. He was ordered an opiate, and saline medicine.

[The tumour afterwards suppurated and burst; and on Oct. 4th, the fortieth day from the operation, and the eighteenth after the ligature had come away, a burst of hemorrhage took place, and he died. On a post mortem examination, it was found that]

The artery on the distal side of the opening was diminished in size. The ligature had been placed on the subclavian, nearly midway between the thyroid axis, and the first of the thoracic branches, which, like those of the thyroid axis, appears to be enlarged. The

ends of the artery divided by the ligature were closed by adhesive inflammation, strengthened by a coagulum in each of half an inch in length, but which did not extend as far as the collateral branch given off above or below it, in this respect differing from the extent to which it has been commonly supposed to proceed. The artery was of its natural size, and gave off six branches between the part on which the ligature was applied, and the opening in the side of the artery into the sac. Three of these were large, one being equal in size to the thyroid axis, and it is my belief that the fatal hemorrhage took place from one or more of these vessels bringing their blood backwards into the artery above the opening into the sac. This was suppurating, and contained only a small coagulum opposite the opening in the artery, about the size of a small walnut, which adhered to its edges, and had in all probability closed up the communication between the vessel and the sac, thus preventing the entrance of blood into the latter. When the adhesions of this clot gave way at its lower and anterior portion, for about half an inch, the sudden and fatal hemorrhage took place. It must be admitted, that although the operation was successful in so far as the application and separation of the ligature, the division of the artery and the adhesion of its extremities, were concerned, it was of very little use in this case. It afforded the patient a chance of life, but it singularly failed in the usually supposed result of obliteration of the trunk of the vessel, even to the next collateral branches. The communication between the branches of the thyroid axis and those on the distal side of the ligature soon becoming free and direct, the circulation to the opening in the vessel was in fact restored, so that from the moment the suppuration caused the skin to give way, the patient's existence depended upon the small coagulum filling up the opening, thus showing the importance of the sac, and the necessity of its remaining entire, in order to ensure success in the operation for aneurism, even when the ligature is applied so closely to the disease as it is usually done in cases of operation on the subclavian artery.

If I had had the good fortune to be present when the bleeding began, I should at once have laid open the sac of the aneurism, now become an abscess, and placed my finger, if possible, on the bleeding vessel, until a ligature could be affixed around it, I should, in fact, have treated the case as one of wounded artery, and which would in all probability have required another ligature at its opposite end.

This case proves in a very remarkable manner the importance of the sac in all cases of aneurism, and the utter inapplicability of the Hunterian theory of the operation for aneurism to a wounded artery, in which no sac exists, an inapplicability which Mr. Guthrie was the first to bring under the notice of the profession, and which he has so strenuously urged in his lectures, lately published, on Wounds and Injuries of Arteries.

The statistics of the various operations recorded in the European journals bear testimony to the unfavourable results of the present

mode of proceeding. I have collected the particulars of forty-two cases, and they demonstrate, that, in the aggregate, the recoveries number two to one failure, that in those cases traced to injury where the ligature was placed in the situation usually selected on the subclavian, external to the scalenus anticus muscle, the failures are nearly equal to the recoveries,—of those in which the axillary artery itself was tied, the whole recovered, as may be seen in the following summary.

Of the forty-two cases, twenty-seven recovered, fourteen failed, and the result of one is not stated. Of the twenty-seven recoveries, fourteen were the result of accident, and thirteen spontaneous, or idiopathic. Of the fourteen failures, eight were traced to injury, and six were spontaneous, and of the whole number more than half were those in which the subclavian was tied, the aneurism originating in violence, so that the following are the results:—

Four cases the result of accident, the axillary artery tied, and the whole recovered. In three of these the sac suppurated, and in one of the latter secondary hemorrhage took place on the nineteenth day, but it was easily suppressed by styptics.

Eighteen cases the result of accident; the subclavian artery tied. Ten succeeded, eight failed. In two of the successful cases the sac suppurated, one in a few days, the other not until three years after the operation. Of the eight failures, the sac suppurated in four, in one, bursting into the pleural cavity.

Nineteen cases of spontaneous axillary aneurism, the subclavian artery tied, thirteen succeeded, six failed. Of the successful cases the sac suppurated in three. Of the unsuccessful cases the sac suppurated in one.

The above, whilst giving the results of the two modes of operation, also shows the relative number of cases in which the sac suppurated in the traumatic and idiopathic form of axillary aneurism; whilst in twenty-two cases resulting from injury we have nine, of nineteen cases originating in disease, we have only four followed by suppuration of the sac.

After the data here given, ought we so indiscriminately to tie the subclavian artery in cases of axillary aneurism as has hitherto been done? Should we not rather tie the axillary artery itself in all cases where the mischief can be traced to a direct injury, and where the size of the tumour admits of our reaching this vessel? Hitherto, whether the aneurism has been caused by a stab, a gunshot wound, rupture by violent stretching, a blow or crush, or by disease, with very few exceptions the treatment has been the same. According to this method, the ligature is applied at such a distance from the sac, that the blood is carried freely into the main trunk between the ligature and the point of rupture; and consequently, in those instances, as in the case above related, where the vessel retains its natural size, and the sac suppurates, the patient must be exposed to secondary hemorrhage, and its lamentable consequences; whereas, if the axillary artery is tied close to the sac, and below the large collateral branches, the free current of

blood into the sac is prevented, so that should suppuration of the latter take place, it would diminish but slightly the chances of recovery as far as hemorrhage is concerned. We have seen the large proportion of recoveries where this practice has been adopted, and been followed by suppuration; in three out of four cases suppuration took place, but all recovered, and in one, although secondary hamorrhage ensued, it was so slight as to be easily restrained by styptics. How different is this from what obtained in the case I have detailed; there the sac suppurated, and hemorrhage occurred, but so violent, that the patient, although receiving immediate assistance, was to all intents dead from the moment it gushed forth.

Lancet, July 7, 1849, *p.* 7.

67.—*On the Treatment of Spurious Aneurism.*—By JOHN ADAMS, Esq., Surgeon to the London Hospital.—[If the brachial artery is unfortunately wounded in performing venesection, the proper practice is by no means, Mr. Adams says, to cut down upon the artery immediately: Mr. A. tells us,]

Proceed in the following manner: roll up a sixpence or any firm body in a piece of lint or soft rag, and place it over the wound, having accurately squeezed the edges of the wound together, and then, by carefully adjusted pressure carried a little above and below the wound in the course of the radial, ulnar, and humeral arteries, to diminish the impetus of the blood, the arm being bandaged to a splint, you may often succeed in completely curing the injury, and in preventing the formation of spurious aneurism. At any rate, you will get a closure of the wound of the integuments, and if an aneurism forms you will then have time to ponder over the necessary treatment of such a case. This treatment is not, however, always to be followed in wounded arteries, many of which it is better to tie at once.

[An aneurism, then, may arise in the following manner:]

A man receives an incised or punctured wound in an artery, and, by the application of pressure, the hæmorrhage is stopped, and the wound heals. In the course of two or three days he perceives a pulsating tumor in the situation of the original wound, and a spurious aneurism is established. In the formation of this aneurism one of two things has happened: either the blood has formed for itself a circumscribed space communicating with the artery, the parts around which have become condensed into a sac, or else a cicatrix has been formed by the exudation of plastic lymph between the edges of the wound in the vessel, and this has gradually yielded, so as eventually to give rise to the formation of a sac, thus composed partly of this expanded cicatrix, and partly by the yielding coats of the artery.

[The treatment of such a case, Mr. Adams thus describes:]

You must apply a compress over the aneurismal sac, and this should extend some distance above in the course of the artery implicated; you are then carefully to bandage the fingers and thumb, and then carry your bandage as high as the axilla, so as to give uniform support to the entire limb, except that a little more pressure is exerted upon the sac and artery connected with it; you direct the patient to keep his arm perfectly quiet, and raise it on an inclined plane: in a short time you will find the pulsation diminish, and the cure will progress. As to the mode in which the cure takes place, it must depend either on the obliteration of the artery at the seat of disease, or else the coats of the artery contract to their original dimensions, and the vessel resumes its natural condition.

If pressure does not succeed in arresting the progress of the disease, the artery must be tied above the tumor, or you may cut into the sac, and tie the artery above and below; for myself I prefer the former method. You must, however, recollect, that even the ligature upon the artery above will not always be sufficient, as considerable pulsation is very likely to return in the aneurismal sac after the ligature; the anatomical disposition of the ulnar artery will readily afford you an explanation of this phenomenon: under these circumstances the employment of slight pressure, after you have taken off the impetus of the blood by the ligature. will in the majority of instances readily effect your object.

Medical Gazette, Aug. 10, 1849, *p.* 222.

68.—NEW INSTRUMENT FOR THE TREATMENT OF ANEURISM.

By Dr. Carte.

It differs from all others hitherto employed, in that the compressing medium is an *elastic* instead of an *unyielding* force. It may be adapted to all cases of aneurism occurring in the extremities where compression can be employed; and, from the trials already made, I have every expectation that it will meet with the general approbation of the profession.

The shape of the framework of the instrument is immaterial so far as regards the adaptation of the compressing force; it may either be in the form of the ordinary clamp, or in that of the ring tourniquet. The latter I consider to be the best, as it is less liable to slip when once placed upon the limb. The following is a brief description of the instrument:—To one side of the framework is attached a well-padded splint, and to the opposite side a ball and socket, or universal joint, which allows the compressing force to be applied in any direction required. Through the centre of the ball (but sliding freely in it) runs a screw about six or

seven inches long, the lower end of which is terminated by a pad for compressing the artery, whilst its upper end is flattened into a handle for the purpose of turning the screw. Half an inch above the ball, but immovably attached to it, is a small iron stage; above this, again, is a corresponding *movable* one, having in its centre the worm of a female screw, through which passes the long male screw bearing the pad. These two stages are united at their edges by two bands of vulcanized India-rubber, in such a way, that when the instrument is applied to a limb, the upper or movable stage (through the medium of the female screw in its centre, becomes a *point d'appui*, against which the elastic force of the vulcanized India-rubber acts; and, by advancing the male screw as the upper stage is raised, a corresponding tension is thrown upon the elastic material. The exact amount of force required for controlling the circulation through the vessel can thus with the utmost nicety be applied, and at the same time the tension can be relieved by simply reversing the action of the screw. This principle may be modified in various ways; for instance, a rack-work, similar to that of the patent corkscrew, might be substituted for the male and female screw.

Medical Times, June 2, 1849, *p.* 604.

69.—ON THE MEANS OF ARRESTING HEMORRHAGE.

By Bransby B. Cooper, Esq, F.R.S.

Actual cautery is, perhaps, next to ligature, the most certain means that can be adopted to restrain the flow of blood from a ruptured or wounded vessel, and can often be employed where a ligature cannot be applied, as in those operations in which bones are implicated, especially the bones of the face; and no surgeon should undertake the removal of a part of the upper jaw, for instance, without being prepared with the proper apparatus for the application of the actual cautery. An instrument which I use for the purpose consists merely of a small rod of polished iron, furnished with a sheath; in the operation, the sheath is to be passed to the mouth of the bleeding artery, and the heated rod then inserted and passed along this canula, until it reaches the artery, which it cauterises without danger of burning the surrounding parts. I once succeeded completely in stopping by this means a severe hemorrhage which followed the extraction of a tooth by Mr. Canton. A frequent method of checking bleeding from this cause is by driving a peg of wood into the alveolar process; but although this plan may prove effectual, it is to be remembered that a foreign body is introduced into the living bone, and may be the cause of much future mischief.

Torsion.—The French surgeons have recommended torsion instead of ligature, when the bleeding vessel is of secondary size; but I have found very little certainty in this treatment, although I believe that failure often results from the manner in which the operation is performed; it is requisite that the bleeding vessel only should be seized by the forceps, and that a sufficient degree of *tension* should be applied to it to tear through the cellular connection; thus depriving the vessel of nutrition by rupturing the vasa vasorum, and at the same time producing lesion of the internal coat: *torsion* is then applied merely for the purpose of destroying the physical elasticity of the outer coat; and if any of thes urrounding tissue be taken up as well as the artery, it will be found difficult to effect the objects required. It may be asked, what are the proposed advantages of torsion over ligature? It does away with the necessity for the introduction of a foreign body into the wound, and consequently avoids the difficulties that subsequently arise in the separation of the ligature. But still I am of opinion that torsion should never be attempted excepting in cases of the smaller vessels. I have frequently found it useful in the extirpation of the mammæ, when the hemorrhage has been troublesome from numerous small arterial branches; and if a ligature had been applied to each of these, a large amount of extraneous matter would have been introduced into the wound.

Compression is a further means of checking hemorrhage; and it is also now emplowed for the cure of aneurism. When used as a means for stopping bleeding, it should not merely be applied upon the wound in the vessel itself, but the bandaging should be so employed as to modify the circulation throughout the whole limb,—as, for instance, suppose a deep punctured wound in the palm of the hand, followed by hemorrhage, which it is thought advisable to attempt to restrain by compression. A bandage should be first applied round each finger separately, and then continued around the hand, a compress being placed immediately over the wound. The bandage should be continued up the forearm, and small compresses of cork placed on the radial and ulnar arteries, with sufficient pressure to diminish without wholly stopping the supply of blood to the hand. The bandage should be continued with a moderate degree of tightness as far as the shoulder.

The patient ought to be kept in the recumbent position, and a low equable temperature maintained in the arm by the application of cold water. A gentle immediate compression may, however, be sufficient without the employment of the gradual impression by bandage.

Medical Gazette, June 8, 1849, *p.* 974.

[Speaking of the manner in which the ligature acts in the suppression of hemorrhage, Mr. Cooper observes:]

Shortly after a ligature has been applied to an artery, the blood begins to coagulate, forming a clot, which extends into the artery both above and below the constricted part: this coagulation takes place with a rapidity proportioned to the diminution of the action of the heart, and is much retarded by any increase in the motion of the blood within the arteries. It occurs more rapidly in the vessels of the extremities than in those nearer the heart, and in small than in large vessels. The length of the clot depends upon the distance of the first large branch from the point at which the ligature is placed: the farther this is off, the longer will be the clot. The small branches leading off near the point of constriction are filled by the coagulated blood, which also often partly fills even larger branches, if these be situated near where the ligature is applied. The clot generally fills up completely the cavity of the constricted vessel, but is not adherent to it, excepting where the internal coat is injured by laceration, in which case the coagulum firmly adheres: it cannot, however, of itself oppose any permanent effective barrier to bleeding from the artery.

In the course of a short period, according to the size of the artery, in consequence of the irritation excited by the clot and ligature, vessels begin to extend into the coagulum from the capillaries of the contiguous coats; a plastic organized substance is then effused; the clot becomes smaller and lighter in colour; it adheres to the internal coat of the vessel, so that it can only be separated by force, and it now begins to assume the appearance and character of flesh. After a time, this organized structure, and the internal coat of the artery, grow completely together, and the danger of secondary hemorrhage is thus precluded. When a ligature is applied to a vein, the coagulum is not formed in the proximal but in the distal side of the ligature: all the other changes (which are similar) take place more quickly than in the arteries.

It must be borne in mind, that the application of the ligature, however dexterously the operation may have been performed, does not always prove efficient to the desired object, and the failure may depend either upon a want of constitutional power in the patient to form the plasma, or upon some physical condition which interferes with the formation of the clot. Indeed, as far as refers to the constitution of the patient, the same considerations would prevail here as in every other description of wound; but the surgeon must employ his scientific knowledge and judgment to obviate the physical difficulties that may present themselves. If after the application of a ligature to an artery, the clot should not be formed from either of the causes mentioned above, secondary hemorrhage would necessarily occur; and perhaps no case can be brought under the notice of the surgeon which offers greater difficulties than the suppressioa of these hemorrhages after the failure of a ligature. This fact wns powerfully impressed on my mind at an early period of my professional career, in a case at Rotherhithe, in which I tied the external iliac artery of a gentleman. I experienced no difficulty in the operation, nor afterwards, until the twenty-second day, when I observed

on dressing the wound, a small fungus-like granulation surrounding the ligature: from this a drop or two of florid red blood issued upon the slight pressure of the plaster placed to support the cicatrix of the external wound, which had now quite healed, with the exception of the opening through which the end of the ligature protruded. The appearance of the excision about the ligature created great apprehension in my mind, and my fears were soon confirmed, for about 12 o'clock the same night, a messenger came for me, stating that a most alarming bleeding had taken place. I immediately accompanied him, but when I arrived the hemorrhage had ceased. I found that the cicatrix had given way, and the whole wound was now filled with a large coagulum of blood. I immediately sent for my friend and then colleague, Mr. Green, who had been present at the operation, and on consultation we decided that the turning out the clot of blood for the purpose of exposing the lacerated extremities of the artery would probably produce a gush of blood that would at once destroy the patient; and as several cases are recorded where, under similar circumstances, the effused clot had itself by its pressure upon the artery produced an inflammation competent permanently to seal the vessel, we determined to trust to the chance of this effect being produced. In this case, however, it failed, and the patient sunk from a recurrence of the bleeding. Upon a post-mortem examination, it was found that there had been no effort on the part of nature to close the vessel: not the slightest trace of clot was found either in the proximal or distal side of the ligature, which had come away at the first hemorrhage; and when a probe was passed up the vessel, it could be distinctly seen shining through the attenuated coats. In this case the mischief evidently arose from want of constitutional power, and I afterwards ascertained that the individual was in the habit of indulging to great excess in the use of wine and brandy; but this was not at all indicated by his general appearance. I of course treated him in the usual manner under such circumstances, but had I been acquainted with his habits, I should certainly not have wholly withheld his accustomed stimulus.

The precautions necessary upon the application of a ligature upon a large artery to prevent the liability to secondary hemorrhage from a physical cause, are—1. Not to place the ligature too near a large branch given off by the trunk to be tied; 2. Not to disturb, more than can be avoided, the surrounding cellular connection to the vessel; 3. To tie the ligature firmly, but not with a force sufficient to cut into the external coat of the artery, which would lead to the separation of the ligature before the adhesive process was sufficiently established; 4. To be careful in the mode of tying the knot in the ligature, so that it cannot loosen (that called the reef-knot is the best suited to the purpose); and lastly, the surgeon should not interfere with the ligature so as to hasten its separation, but patiently leave it to nature's own process. The period at which the separation will occur, will differ according to the size of the artery, and the peculiar constitution of the patient.

An interference on this point is the more reprehensible, as all the statistical accounts of secondary hemorrhage prove that on their occurrence the danger is diminished in proportion to the length of time that has elapsed after the tying the artery, and that few prove fatal after the lapse of between thirty and forty days; but notwithstanding all these precautions, the application of a ligature may prove abortive, in consequence of some abnormal distributions of the trunk upon which the ligature has been placed.

Medical Gazette, June 22, 1849, *p.* 1057.

70.—*On Aneurismal Varix.*—By Bransby Cooper, Esq., F.R.S. —[Aneurismal varix, or a communication between an artery and vein, occurs. Mr. Cooper observes, not merely from accident, but may arise from disease, and may be found between any artery and vein lying in close proximity. Mr. Cooper remarks:]

An aneurismal varix, from whatever cause it may arise, may be recognised as a blue, circumscribed, pulsating tumor, lying in the course of a large arterial trunk. Upon auscultation, a distinct whizzing noise is heard at each systole of the heart; while a less distinct whizzing sound is distinguishable in the diastole when the tumor becomes comparatively flaccid. Pressure upon the artery above the tumor stops the pulsation, and the swelling becomes flaccid, but instantaneously re-fills when the pressure is removed. The swelling may be easily emptied by direct pressure upon it; the veins below the swelling are generally enlarged and tortuous, and the circulation through the artery impeded. The tumor rarely increases much in size, seldom exceeding that of a small walnut, unless much exertion is employed, to which generally the patient has great intolerance, in consequence of the weakness of the limb, attended with more or less pain—inseparable concomitants of this condition.

The most frequent cause of aneurismal varix is the transfixing the vein and wounding the artery behind it in the operation of phlebotomy. This accident is immediately indicated by a profuse rush of arterial blood gushing per saltum through the wound. The removal of the ligature placed on the upper arm preparatory to venesection does not check the bleeding, which can be stayed by compression made upon the wound by dossils of lint maintained in their position by bandages. I do not myself, however, consider this a good plan of procedure, as, in order to stop the bleeding, the bandages must be applied sufficiently tight to impede the circulation in the lower part of the limb, which causes swelling and pain, so that it is generally necessary to remove the bandage, when hæmorrhage will be certain to recur. It is better, in my opinion, to apply a graduated bandage along the whole of the arm, commencing at the ends of the fingers separately, and continuing it up to the shoulder, placing at the same time a compress upon the middle portion of the brachial artery with sufficient firmness to

check, without stopping, the arterial current. Under these circumstances a tendency to coagulation of the blood is set up, and is further promoted by the patient being kept in the recumbent posture, with the arm elevated on a pillow, and kept constantly wet with cold water.

About four years ago, a young surgeon of my acquaintance, in bleeding a patient in whom the vein was immediately over the artery, opened the vessel with such precaution that a friend who was present at the operation said to him, "You'll never get half blood enough from that opening," and, taking the lancet from him, made a second puncture, upon which arterial blood gushed forth with great violence. The cautious operator immediately compressed the opening, but found it extremely difficult to prevent the flow of blood until the brachial artery was well compressed above. He then proceeded to bandage up the whole arm, commencing with the fingers separately, and filling up the bend of the elbow with lint, particularly placing a dossil of that substance over the wound, and, at the same time, placing a second compress on the middle of the brachial artery, to retard the flow of blood through that vessel. Under this treatment, the wound through the vein and artery perfectly healed, and this took place apparently without the obliteration of either vessel, as the pulsation was afterwards as distinct as is natural at the wrist, and the superficial veins retained their normal appearance. If this mode of compression fails, the communication between the artery and vein remaining permanent, the wound through the skin only having healed, an aneurismal varix is established, and will require further surgical treatment for its cure. Sir Astley Cooper, in speaking of aneurismal varix, says, "that no surgical operation is generally required in this disease, as it is not dangerous either to life or to the arm." Such may be the fact to those who have no necessity for the violent exertion of the limb; but to a poor man who has to gain his bread by his own exertions, such a limb would be totally incompetent, not only from its weakness, but from the danger arising from the tumour being exposed to external injury. There are two modes by which this abnormal condition may be removed, viz., either by tying the brachial artery in the middle of the upper arm, or by laying open the aneurismal sac, and tying the artery above and below the point of lesion. If the first operation be chosen, it may easily be effected by making an incision through the skin, two inches and a half in length, in the middle of the upper arm, on the inside of the biceps,—thus exposing the fascia of the arm, which is to be laid open to the same extent, when the biceps muscle is exposed, and is to be drawn outwards. The median nerve, brachial artery, and venæ comites, are exposed; the artery is to be separated from the nerve and veins, and, a ligature being passed around it, it may be secured in the usual manner. This operation does not, however, always succeed, in consequence of the free recurrent anastamoses between the radial, ulnar, interosseal, and inferior branches of the brachial arteries. I nevertheless believe that this operation would rarely fail if due pre-

caution were taken to apply gentle compression on the fore-arm, as before described, in addition to the ligature applied to the brachial artery.

Should the aneurismal varix prove, however, incurable by these means, the following operation may be had recourse to; but it is certainly a far more dangerous one, in consequence of the cluster of veins and nerves which are superficially placed at the bend of the elbow, besides the close proximity of the division of the brachial into the radial and ulnar arteries.

I have myself successfully performed the following operation, as have also my colleagues, Mr Key and Mr. Morgan. The mode of performing the operation is as follows:—A tourniquet is placed upon the brachial artery, so as completely to compress it, and free incision is then made into the aneurismal sac, and, the clot of blood being turned out, the interior of the sac cleansed by a sponge, the upper and lower opening of the artery into the sac is then to be sought for, and the end of a probe passed into the upper opening, to serve as a guide in separating the artery from the surrounding tissues; after which the vessel is to be secured. The same process is then to be repeated upon the distal end. This operation is frequently followed by constitutional irritation, and more so than the former operation, although it must be admitted that its effectiveness is far more certain.

Medical Gazette, June 22, 1849, *p.* 1063.

71.—ON ANEURISM BY ANASTOMOSIS.

By Bransby B. Cooper, Esq., F.R.S.

[Aneurism by anastomosis, or nævus maternus, as it is termed when congenital, consists, as is well known, of a congeries of enlarged and dilated arteries and veins, which can be emptied of their contents by pressure, and gradually refill when the pressure is removed. Mr. Cooper considers the disease to be of an *atonic* character, dependent, not upon excessive action, but upon a defective condition of the coats of the vessel, arising from arrest of nutrition; so that the disease may be said to originate in the vasa vasorum. As to the treatment of this affection, Mr. Cooper says,]

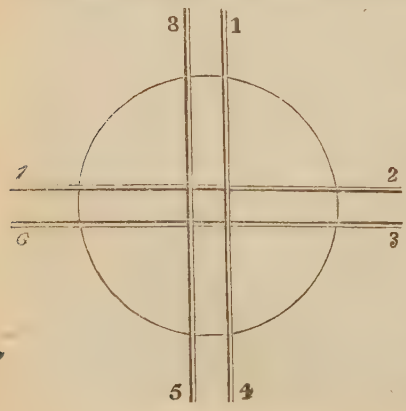
These tumours should be at the earliest possible period subjected to proper surgical treatment, as the danger and difficulty of cure are increased in proportion to the size they are permitted to attain. In infants the affection is generally congenial, occurring as nævus, and at first is so superficial and small in size, and differs so slightly from the surrounding parts in colour, that it scarcely possesses the physical characteristics of aneurism by anastomosis, and seems to be, indeed, a mere hypertrophy of the rete mucosum, being usually situated in the integuments of the face, neck, or some other highly vascular part. These spots sometimes disappear spontaneously, and if but small and not much raised above the skin, I have known them removed by merely keeping the surface constantly wetted

with a strong solution of alum. If this remedy prove effectual, it will be known by the nævus becoming hard and colourless, as if its blood had become coagulated: nitric acid is also sometimes employed, but it produces a slough, upon the separation of which there is great liability to hemorrhage, and as children bear the loss of blood very badly, I do not consider the use of this powerful agent safe. Pressure upon the cellular tissues surrounding the nævus is sometimes resorted to, the intention being to cut off the supply of blood to the part: the best way of effecting this is to make a plaster of Paris cast of the tumour, and when this has become hard, to bind it upon the part with sufficient force to prevent its receiving nourishment from the surrounding vessels: as the size of the tumour decreases, thin layers of lint must be placed in the plaster mould, to enable the pressure to be kept up; but this plan of treatment can only be adopted when the swelling is situated over bone, as in the scalp; and many years ago I remember a case being thus successfully treated by Mr. Hodgson, of Birmingham, who then lived in London. I have twice adopted this plan, but unsuccessfully, and was obliged to proceed to other means. Injections into the erectile tissue are sometimes employed: weak solutions of iodine, corrosive sublimate, and alum, have all been used, and with success. I have myself several times injected nævi with a saturated solution of alum, as in the following case:—

A child of a poor woman, about six months old, was brought to me with a nævus near the ala of the nose, extending to the corner of the mouth: it was prominent, compressible, the skin of a strawberry red colour, and the growth of the tumour had been very rapid. I procured a "lachrymal duct syringe," and making a very small opening in the healthy skin, close to the circumference of the tumour, injected half a drachm of the solution of alum into the erectile tissue: the colour of the skin was immediately changed to a blue or slate colour. I desired the surface to be kept constantly moist with a solution similar to the one I had injected, and the disease was soon entirely removed, without a single bad symptom. Mr. Longmore, jun., of the Borough Road, sent me a case of a nævus, in the side of the cheek, which I inoculated with vaccine matter,—a plan which has been said to prove effectual in curing this disease: the treatment proved, however, unsuccessful in this case, and I injected the tumour with alum, and treated it as in the case described above, with complete success. In several other cases I followed the same plan, with the happiest results: in all these, with one exception, the nævi were on the head and face, and that was in the arm. After this series of successful cases, I was consulted by the wife of a clergyman in Suffolk, whose child had a large nævus on the inner side of the thigh: the tumour was very compressible, and communicated more than usual thrilling sensation, as the blood was circulating through the tissue. With the confidence inspired by my former success, I proceeded to inject the tumour with a solution of alum, as before described, but employed a larger syringe, in consequence of the greater size of the tumour.

The child did not at the time appear to suffer more than is usual in such cases, but towards the evening of the day of the operation I was sent for, in consequence of its excessive uneasiness. I found it suffering under a high degree of irritative fever: the thigh was much swollen and inflamed. I immediately ordered a dose of calomel and antimony, and applied fomentations to the limb; but for three or four days the child remained in the most imminent danger. The aneurismal swelling then sloughed, and a deep and extensive wound was the result; but the child's powers of constitution carried it through both the acute attack and the protracted process of reparation. I have no doubt that in this case the injected fluid became immediately mixed with the blood, and reacting upon that fluid, either produced some change in it, rendering it unfitted for the purposes of life, or otherwise inflammation in the veins, termed phlebitis. I have never since ventured upon the repetition of this operation, and am now, upon mature consideration, more surprised at the fortunate termination of the former cases, that at the result in that last described; for what can be more likely to lead to extensive constitutional mischief than the probable immediate introduction of a powerful chemical agent into the blood? I have therefore decided upon never again venturing upon injection where the nævus is of large size.

[Mr. Cooper does not appear to approve of the plan of excising nævi, which he thinks dangerous on account of the hæmorrhage which may follow. He thinks the application of a ligature to be the proper method of treatment. If the tumor is small, two needles may be passed through its base at right angles to each other, and a ligature tied tightly round them: as the tumour ulcerates, the ligature will become loose, and must be replaced by another. But in the case of large nævi, a different method is to be practised.]



The late Mr. Liston always employed, and I think first proposed, a most ingenious arrangement of ligature for the constriction of large nævi; in this operation the tumor is first raised as completely as possible from the subcutaneous structures; a long needle fixed in a handle, but without a ligature, is then passed through its base below the diseased tissue; a second needle, threaded with a double ligature, is passed in a

similar manner, but at right angles to the first; the first needle is next threaded, and both then withdrawn, leaving the ligatures in the tumor; the loop of each double thread is next cut, so that four single threads now cross the swelling, each with two free extremities (see Fig. in which the extremities are numbered for reference.) In securing this system of ligatures, extremities 1 and 2 are first tied, without being tightly drawn; 3 and 4 are next tied, 6 being held by an assistant to prevent its being drawn through; 5 and 6 are then secured, 8 being held; and, finally, 7 and 8 are tied as tightly as possible, and as they command all the others, the degree of constriction depends entirely on the manner in which they are secured.

I have myself employed this ligature in four or five cases, and am convinced by experience of its great utility.

Medical Gazette, May 25, 1849, *p.* 884.

72.—*Treatment of Nævus by a Solution of Iodine.*—By Dr. J. O. Bulteel, Plymouth.—[Dr. Bulteel has used with the most satisfactory result, a solution of iodine ℈j., in sp. vin. rect. ʒss. He says:]

The preparation was applied freely once every day, not exciting the slightest constitutional derangement, and the disease every two or three days scaling itself, if I may use the expression, and thus disappearing *gradatim* till nothing more could be seen but two little spots of the size of a pin's head. The application at once arrests the growth of the nævus, and nothing but a regular daily application is needed for its final removal.

Medical Gazette, Aug. 24, 1849, *p.* 319.

73.—*On the Effects of Chronic Arteritis.*—By Bransby B. Cooper, Esq., F.R.S.—[One of the most common effects of chronic inflammation of arteries is thickening of their coats, which by impairing their elasticity, is no doubt a frequent cause of rupture of the inner coat, and the consequent formation of aneurism. Further:]

A thickening of the valves of the aorta, in all probability proceeding from chronic inflammation, is very frequently met with; they are sometimes converted into a kind of ligamentous tissue, and thus become completely unfitted for their original important office. This thickening sometimes goes on to such an extent as to create wart-like excrescences, which Corvisart terms "vegetations."

Chronic inflammation sometimes produces atheromatous deposits between the internal and middle coats of an artery, forming projections into the canal so as materially to interfere with the flow of blood through the vessel. It has lately been proved by means of the microscope, and by chemical analysis, that these morbid growths consist of fatty matter, and therefore steatomatous deposit would

perhaps be an appropriate term for the disease. The obstruction offered in this disease to the circulation of the blood often produces distension of the vessel above the deposit, and in some cases becomes an exciting cause of aneurism. These deposits are not by any means uncommon, as the numerous preparations in our hospitals serve to prove; its diagnosis during life is very difficult, unless it be situated where auscultation can be employed in the investigation, as in the aorta; in that case a bruit would be present from which the true cause of the obstruction could generally be conjectured.

Bony deposits also frequently result from the effect of chronic inflammation; and, indeed, this deterioration is so common in the arteries of old people that it must be regarded, properly speaking, as a change inseparable from senescence, and not as disease. The cause of this action is probably to be traced to a diminution in the power of the capillaries of the bones to eliminate phosphate of lime from the blood: an undue quantity of that earthy matter being therefore carried into the circulation, it is deposited in the more vital parts of the system. Under certain diseased conditions young persons are liable to the formation of these deposits of bone-earth, or ossification of arteries as it is termed.

A frequent result of ossification of the arteries, especially when it occurs in the lower extremities, is sphacelus of the limb, in consequence of the imperfect nutrition which the rigid arteries are capable of supplying to parts distant from the heart; it is therefore very desirable to prevent wounds of the toes or extremities in old people, and also to maintain, by topical means, the normal temperature in the limbs. Ossified arteries are liable to disruption; the vessels of the brain sometimes give way under these circumstances, inducing coma, to which, I think, the term "senile apoplexy" may be aptly applied.

The presence of ossification of the arteries may be diagnosed, particularly in old people, by the irregularity of the circulation, and, when when the vessels of the extremities are affected, by the tangible rigidity of the diseased vessel. Ossification of the coronary arteries of the heart produces a train of symptoms which constitutes the well-known disease termed "angina pectoris." Gouty and rheumatic diatheses seem to be especially liable to morbid depositions in the arteries; it is therefore highly important, in acute attacks of rheumatism or gout, to entirely subdue the specific action as early as possible, so as to prevent it from passing into the chronic form, under which there is always considerable tendency to the various depositions I have described, all of which are more or less likely to produce aneurismal dilatation.

Medical Gazette, May 25, 1849, *p.* 883.

74.—ON THE OPERATION OF TRANSFUSION OF BLOOD.

By Dr. C. H. F. Routh, Physician to the St. Pancras Dispensary.

[Dr. Routh has collected together all the recorded cases in which transfusion was performed, and from these data has come to conclusions very favourable to the performance of the operation. After enumerating the cases, he observes,]

From this list we conclude—1st, That the operation is one of the safest major operations which may be practised in surgery.

The total number of cases here given are 48; the number of deaths, 18; giving a rate of mortality of 1 in 3—rather less than that of hernia, or about the same as the average of amputations, according to some authors.

A very little consideration, however, of the table will satisfy us that this average is too high. In all these 48 examples, it is true, the operation of transfusion was performed, but in some inadequately, and in others where there was no reasonable hope of deriving any benefit from it. In other of the cases, death was certainly due to other causes than the operation.

1. In cases 5, 6, 12, and 16, the patients were *already dead* when the operation was performed.

2. In case 8, as will be more especially noted in the sequel, enough blood was not transfused.

3. In cases 7, 9, 15, 21, and 48, there was other mortal disease present: so that, although at first some benefit resulted from the operation, this good effect was but transitory.

4. In case 44 the operation was unfortunate, air having been inadvertently admitted in the veins.

And lastly, in cases 45, 46, and 47, the same objection applies as in case 8.

Thus, out of these 18 deaths, we have only 4 in which death can be reasonably attributed to the transfusion, i.e., out of 34 cases in all we have only four deaths, or 1 in 8½.

This result, however, is scarcely correct, though it accords with that obtained by Mr. Peet. This gentleman, in a paper read before the University College Medical Society, now unfortunately lost, but an abstract of which appears in the Lancet, (1842,) came to the following conclusions. He collected 35 cases where transfusion had been performed. Of this number 13 died—an average mortality of 1 in 2·7. But he also remarked, that this result was incorrect, as among the fatal cases in 3 only could death be referred to a failure of transfusion, giving thus a total of 25 cases, of which 3 only were fatal, or 1 in 8·3.

I do not think, however, that the four first cases given in the Table should be included in this average. The accuracy of the record of these may and has been disputed; but, moreover, as the operation in these cases, excepting in the last, was not performed upon diseased persons, the facts only go to prove that transfusion,

and that even with the blood of animals of a different species, may sometimes be practised upon comparatively healthy persons with impunity. Excluding, therefore, these 4 cases, we have 30 cases, of which 2 only were fatal, or 1 in 10. This is a highly favourable result; more so, indeed, than that obtained in the other major operations, if we except that in lithotomy, obtained by Saucerot.

2. In reference to the diseases in which the operation has been performed.

a. In uterine hemorrhage. The result is very flattering. It was performed in twenty-four such cases. In five the patient did not recover. In one only, however, could the operation be reasonably stated to have failed. In two cases (Nos. 5 and 12) death had already occurred. In one (8) sufficient blood could not be obtained. In another (44) air was accidentally admitted into the vein. In one only the benefit produced was transitory, and it failed (35.)

In four cases the hemorrhage was secondary, coming on and continuing for some days after delivery. In all these it proved successful. (Cases 13, 34, 41, 43.)

In 2 cases the operation was performed before labour, and in both with a happy result.

b. After surgical operations, transfusion was performed 7 times (6, 20, 21, 28, 32, 36, 42), in three cases only proving fatal (6, 20, 21.) In two of these fatal cases, however, (6 and 21,) death is accounted for by other causes. In both these cases the operation was performed for secondary hemorrhage. The absence of a post mortem in case 20, prevents the same conclusion in regard to the cause of death being stated with any degree of certainty.

c. It was twice performed in the state of exhaustion following continued dyspepsia. In one it was fatal, but the post mortem here revealed organic disease, scirrhus of the pylorus. The other case was certainly cured by the operation.

d. It proved successful in one case, under the most unfavourable circumstances. In a case of phthisis, with violent hæmoptysis; and this is the more interesting, as the blood of a *ram* was the fluid used in this case. The quantity, however, transfused was small, only 6 oz. It is, however, a fact of some interest; the more so, if taken in connexion with the cases 1, 2, and 4, where sheep's and calf's blood were employed; and must be looked upon as an instance of the power of the "*vis medicatrix naturæ*," where a patient got well in spite of the remedies employed, or as confirmatory of Dr. Harwood's experiments, and opposed to those of Drs. Blundell, Dieffenbach, &c.

e. It proved of no avail in one case of hydrophobia, and another of asphyxia.

f. It proved fatal in 3 out of 4 cases of Asiatic cholera. In the 4th it probably exerted a beneficial effect: probably that effect might have been permanent, but for the existence of other organic and mortal disease. In the other 3 cases, the quantity transfused was insufficient. In cholera especially, where the amount of fluid that runs away by the bowels is so great, we should expect *à priori*,

that no advantage could be derived by the transfusion of a small quantity; the more so, as in those cases where injection of saline substances has been tried, as much as 100 oz., and 120 oz., and even upwards, have been transfused, with the best effect. This view of the case obtains apparently confirmation, from a simple review of the history of the four cases in which it was tried. Life was prolonged in proportion as the quantity injected was large. In case 45, $1\frac{1}{2}$ oz. only were injected. He died in six minutes after the operation. In case 47, $2\frac{1}{2}$ oz. were injected: the patient died two hours after the operation. In case 46, 6 oz. were injected; the patient died in six hours after the operation. In case 48, 30 oz. were injected, and death resulted in sixty-six hours. In the two last cases, (46 and 48,) in the first, the cholera symptoms did not recur; in the last they were overcome. The injection of larger quantities in cholera derives further confirmation from the following calculations.

3. As a rule, the quantity injected, where transfusion is practised, should not be under 7 oz.

It is admitted, that a lesser quantity may, and has been successfully transfused. Thus, in seven cases in the table, a cure followed under these circumstances:—

In case 22	there were transfused	2 oz.	
„ 23	... ...	2	
„ 28	... ...	5	Mean
„ 37	... .	6	3·8 oz.
„ 38	... ...	4	Say 4 oz.
„ 39	... ...	4	
„ 40	... ...	4	

But, on the other hand, where the quantity transfused before any symptoms of rallying took place, is distinctly stated in the table, we have the following result:—

In case 10	there were transfused	6 oz.	
„ 11	... ...	6	
„ 13	... ...	6	
„ 17	... ...	8	
„ 19	... ...	5	Mean
„ 26	... ...	8	7·0 oz.
„ 28	... ...	5	
„ 31	... ...	12	
„ 40	... ...	4	

Thus it is, that we explain the failure of the operation in cases 8, 45, 46, and 47, a sufficient quantity not having been transfused. This result is still more certainly brought out in another table, where the fatal and successful cases, and the quantities transfused, are opposed for the purpose of making this more evident, arranged in separate columns, beginning with the smallest mean quantity transfused in successful cases, one column being set apart for every additional 4 oz. up to 16. In this table, the 4 first cases, and the 4 cases in which death had already occurred, when the operation was performed, are excluded:—from which it appears, that the greatest

proportional number of successful cases are those in which a quantity of not less than 12 oz., or more than 16 oz., was transfused; the number of successful cases proportionally increasing up to this limit, and then diminishing again, giving a mean of 14 oz., as the quantity to be transfused.

The same result is brought out, though more generally, if we arrange these series of cases in two classes only; the one, where the quantity transfused was under 12 oz., and the other where it exceeded this number:—*i.e.*, we have in the former class 8 deaths, with an average of 5·3 oz. transfused, and 14 recoveries, with an average of 5·9 transfused, or 1 death to 1·7 (or nearly 2) recoveries, and in the latter class we have 3 deaths, with an average quantity 20·1 oz. transfused, and 9 recoveries, with an average 16·5 oz. transfused, or 1 death to 3·2 recoveries.

From which we think it follows, that, as a rule, the quantity transfused should not be less than 6 oz., nor more than 16 oz.

4. In regard to the age of the patients in which this operation was performed, the data are insufficient in the fatal cases. In the 16 successful cases in which the age is given, the mean age was 34·8. In 3 cases the age was under 30; in 4, under 35: in 5, under 40; and in 3, under 42.

5. The danger of the operation is certainly not great. Among the successful cases, phlebitis, which *à priori*, we should most fear, occurred in only one case, case 37, where the blood was taken from a *ram;* but even in this case it was easily overcome. In the only 3 cases where we have seen, after transfusion, death followed (cases 20, 35, and 36), the operation could only be said to have failed to cure, but not caused death; and even in one of these, (case 20,) Dr. Blundell expressed an opinion that there was some other latent cause of death.

6. In regard to those diseases in which this operation might be performed, I think there are many in which, by reason of a mistaken timidity on the part of the practitioner, it has not been practised; and where the probability is, that if the contrary course had been pursued, life might have been spared. Too much hardihood is certainly very blameable, but timidity is scarcely less so. The misfortune is, however, that death might be frequently improperly ascribed to the operation, if it failed, and more especially, as it might be improperly performed. With the state of public feeling, therefore, it can at present only be resorted to in extreme cases. We can, therefore, only speak of it as applicable in those diseases where collapse is present or imminent.

From the table we learn that out of the 18 cases in which it failed or proved fatal, (in 5), other organic disease was present. In only 2 cases where there was or had been organic disease, did it prove successful. While, therefore, these figures do not absolutely forbid the employment of transfusion in organic disease, they very much discourage it. Common sense would necessarily contra-indicate its practice, especially in diseases of the lungs or heart.

But, first, in all cases of collapse induced by hemorrhage, whether primary, or secondary, it should always be employed.

Secondly, in that state of extreme exhaustion from dyspepsia, where collapse is imminent, it might be advantageously employed. There are many cases in which dyspepsia has persisted for a long time, and perhaps been neglected, and where, notwithstanding, no organic disease can be detected; in which no food can be retained on the stomach, and the remedies employed are powerless. Here the patient will sink from inanition, if something is not done. In these cases it is highly probable, that were the operation more frequently resorted to, life might often be saved. Dr. Blundell kept a dog alive three weeks, by consecutively transfusing the blood of another dog in his veins. It is true, in this case the animal lost weight, and was evidently thinner at the end of the time: but the operation, once performed in a full and effective way, might enable the patient to rally, so that other remedies might subsequently have effect.

Thirdly. In some cases of stricture of œsophagus, where no food can be taken, to give the patient time to rally, so as to admit the introduction of bougies subsequently.

Fourthly. In the collapse which follows long-continued fevers, more especially those of a typhoid character, produced frequently by supervening diarrhœa, or by the crisis of the disorder. It is exceedingly probable, transfusion here might be most advantageous. In that variety of Irish fever, so fearfully fatal, and where the crisis is marked by excessive perspiration, in the exhaustion that invariably follows, and from which the patient so seldom recovers, it might be successful, giving the patient time to rally; for then the digestive organs are too often too slow and too weak in their operation to act with effect, whereas a sudden change in the quality and richness of the blood would probably save the patient. But even after the crisis, in a more advanced stage of the convalescence, death not unfrequently results, after a continued fever, from progressive weakness, and here transfusion might probably effect a cure.

Fifthly. In the collapse, or great exhaustion, following diarrhœa, dysentery, and cholera, where the use of injections especially would be contra-indicated, or useless, it might be, probably, advantageously employed. Among children especially, diarrhœa is a very common cause of death, from the mere exhaustion which it produces, although, it is true, in some of these cases, there may be organic disease, which would neutralize the good effect of transfusion; but even in these cases, sometimes, it might do good, by giving time for subsequent and successful treatment.

The same remarks apply to the exhaustion after sporadic cholera. In many of these cases, fatal syncope is at once induced by the patient raising himself in bed, even after all the active symptoms of the disease have subsided, denoting great weakness, which might be effectually combated by transfusion.

Lastly. In Asiatic cholera. In the state of collapse, whether by reason of the nervous shock induced or not, we can trace two, at east, efficient causes. 1. Diminished fluidity of the blood, pro-

duced by the violent watery purging; but, 2. it would seem, from the non-secretion of urine and bile, that some essential vital principle in the blood (necessary for the stimulation of that part of the nervous centres connected with these organs, or the particular nerves of the organs, or with their secreting cells themselves) is either so removed, or so changed in kind, as no longer to produce this stimulating effect. That it is not always so removed, is proved by the fact, that the transfusion of saline substances has sometimes cured the disease; but that it is frequently so, is also proved by the frequent failures of this mode of treatment. If this view be correct, to restore this principle becomes the first indication in the treatment. We do not know what it is. But the transfusion of blood, which, as blood, does contain this principle, would seem the best calculated to fulfil this indication.

In the four cases of cholera given in the table, we have some confirmation of this view. In Cases 45 and 47, the quantity transfused was too small to produce any change. In Case 47, where six ounces were transfused, life was prolonged six hours; and in Case 48, where thirty ounces were transfused, the choleraic symptoms disappeared, bile being secreted, and life prolonged sixty-six hours. It is impossible to ascribe, upon the evidence of one case, these amendments to the transfusion. More experiments are required, but the coincidence is interesting.

The objections to transfusion in Asiatic cholera seem to be twofold. 1. The difficulty of obtaining a sufficient quantity of blood. In the cases treated and cured by transfusion of saline substances, as much as 100 to 320 oz., and upwards, were sometimes injected. To inject this quantity of blood would be impossible, but it is probable a smaller quantity of blood would suffice; and if not, the transfusion of blood might be combined with the transfusion of saline substances, the one operation preceding the other.

2. The second objection is the fear of contagion; but, even here, kind friends will be frequently found willing, at any risk, to save the life of a relative; and here we must depend upon the feelings and affections of others. How far these theoretical suggestions are correct or false, can only be proved by further experiments. The dreadful havoc now produced by the cholera on all sides must be the excuse for their promulgation.

Sixthly. It remains only now to add a few words on the method of performing the operation itself, and I cannot better do so than in quoting the very words of my friend and colleague Mr. Walton, who has kindly furnished me with the following remarks on the subject:—

"The operation may be executed in three ways—1. By receiving the blood in a vessel, and allowing it to enter the body by gravitation. 2. By interposing a tube between the blood-vessels of the emittent and recipient persons, and trusting to the force of the circulation for its transmission. Both these methods have given place to the 3rd or injecting process, which is effected by the stop cock stomach pump; but, instead of the tube through which the pump

would be filled, there is a funnel-shaped basin to receive the blood. Much nicety is required in its make.

"So little is the theory of transfusion understood by instrument-makers in general, that instruments are made and sold for the purpose that are wholly inapplicable. Mr. Coxeter furnished me with mine. Mr. Ferguson, of Smithfield, has produced the most perfect one that I have seen, The basin should be cased like a hot plate, that it might contain water. The piston should fit accurately, and at the same time work freely. I think that an interruption by some elastic material in the tube that is to convey the blood, is advisable, for then the pipe that is inserted into the vein is less likely to be disturbed by any motions which may arise in the working of syringe or movements of the patient. At least three persons are required for the safe and efficient performance of the operation, and their attention should be wholly given to it. Supposing everything ready, the instrument quite clean, air-tight, and thoroughly warmed, the basin filled with hot water, a vein in the patient's arm should be punctured, and the finger applied to the orifice till the tube is ready to be inserted. It is quite unnecessary to dissect out a vein, and place a probe under it before making the puncture, as has been recommended. A vein in the arm from which the blood is to be taken, is next to be opened by a fine cut, and the stream directed into the centre of the basin, the arm being held close to it. When sufficient has entered, the syringe should be filled, and the piston pushed in a little till blood flows at the end of the pipe, which is then to be introduced into the vein. The injector now works the syringe till the desired quantity has been thrown in. There should be no cessation to the stream of blood entering the basin till the operation is ended, so that, after each occasion of the syringe being filled, there may be at least an ounce of blood at the bottom of the basin, or else air would probably be sucked in. It will be perceived that I have given merely a hasty sketch, but sufficient, I think, to guide those who have no experience in the operation. The accident to guard against is the transmission of air. The greater facility with which this is likely to occur when transfusion is performed through the jugular vein, points out the impropriety of selecting this vein for the operation. It would be worse than useless to attempt to transfuse unless an expert bleeder be procured. Much of the success of the operation depends on the despatch, as well as the steady and uninterrupted course that is pursued; for, with the disadvantage of the blood travelling over dead surfaces, very little exposure of it to air must be apt to destroy vitality.

The above account has been given, as the subject of transfusion is usually excluded, in works even of Practical Surgery, although much simpler operations are treated in detail. Even Cooper's Dictionary 'is silent on the subject.'"

Medical Times, Aug. 11, 1849, *p.* 114.

ALIMENTARY CANAL.

75.—ON THE EXTRACTION OF FOREIGN BODIES FROM THE ŒSOPHAGUS.

By Professor Dieffenbach.

Foreign bodies observe in their transit certain *stations* at which they halt; thus, in the pharynx behind the thyroid and crycoid cartilages, in the beginning of the gullet, or at its lower end, close to the diaphragm or cardia. They seldom stop at the middle of the gullet. If very large, they may cause suffocation—thus a large piece of meat, a hard boiled egg, a pear, a chestnut, have each proved fatal. Guattani witnessed the most frightful death ensue from a chestnut: the part of the gullet at which it stuck was gangrenous. Spiritus saw the same result follow the swallowing of a five franc piece, which perforated the gullet above the cardiac orifice. Needles, inadvertently swallowed, pierce sometimes the gullet or stomach, advance by the aid of suppuration or otherwise towards the surface, and either escape spontaneously, or through incision. Lyson observed a case where three needles that went in at the mouth came out at the shoulder; I have known one issue at the arm.

The procedure must be modified according to the nature of the substance. None but a bungler would attempt to disgorge a piece of meat sticking at the cardiac opening, or urge on a fragment of glass from the gullet into the stomach. External pressure will suffice for potatoes or plums when stuck in the throat.

For the withdrawal of needles, fish bones, and the like, there is no better implement than a large goose or swan quill feather, with the barbed portion ruffled, and imbued with oil. The patient sits with his head leaning upon the breast of an assistant, while the surgeon lowers the tongue, then introduces the feather, with its concave side downwards into the throat, turns it rapidly round, and draws it out. The popular practice of swallowing a crust of bread is sometimes availing, but may also increase the peril when arrested above the bone. A sudden slap on the back is by no means a bad plan, when the substance is large and obtuse. It is preferable to that of setting the patient on his head, as was done in the instance of Mr. Brunel to promote the expulsion of the half-sovereign piece.

The principal instruments employed for the present purpose are of the description of forceps. Dupuytren advises, as a preliminary step, the introduction of a gum elastic tube, surmounted with a silver ball, in order to ascertain the position of the foreign body. This, however, is superfluous, and will tend, moreover, to augment irritation. Cooper recommends the forceps of Weiss. The so-called leaden hammer of earlier writers consisted of a lead ball attached to a string, which was let down the throat

and pulled up again. Mesnier's lead hammer was of an olive shape; Petit's was equipped with a wire instead of a string. Petit used besides a metal noose fastened to a whalebone stem; Fabricius Hildanus a many-holed silver tube provided below with a sponge. The double ring of Graefe attached to the end of a rod of whalebone with a steel spring, is very convenient for taking pieces of money out of the throat. The customary instrument, termed *repoussoir*, or probang, namely, a bit of sponge as big as a walnut stuck to the end of a whalebone, is generally useful, either for entangling fish bones and the like, or propelling large round substances. My own procedure is as follows: if the body be small and sharp, I employ the oiled feather above described. An oiled wax taper, passed down to the cardiac orifice, has proved serviceable; for, as soon as withdrawn, the body has been rejected. If the body be large, as a portion of flesh meat adherent to a fragment of bone, I use a lithotrite with an imperforate scoop, and rather straight. The instrument is introduced with the blades closed, until it arrives at its destination, when these are to be separated sufficiently to grasp the substance, and, after a few gentle turns, withdrawn.

When there is impending suffocation from the presence of the very large bodies impacted in the throat, Habicot enjoins tracheotomy before resorting to opening the œsophagus. I have never been compelled to this extreme measure. The most difficult thing to deal with are sets of false teeth when swallowed. I once relieved an old lady in this predicament by means of my fingers. On several occasions I have removed, with curved polypus forceps from three to four teeth attached to a gold plate, and which got accidentally into the throat; once, by the aid of an emetic, as a last resort, a set of four teeth very deeply located.

In all these operations, the patient is to be in a sitting posture; the head properly supported, the mouth rinsed with tepid water, tepid water mixed with white of egg taken as a drink, and the instrument smeared with white of egg rather than with oil.

Medical Times, July 28, 1849, *p.* 78.

76.—ON PETIT'S OPERATION FOR STRANGULATED HERNIA.

By H. Hancock, Esq., Surgeon to Charing Cross Hospital.

[Mr. Hancock is opposed to the practice of operating for hernia without opening the sac. He thinks that it is proved that the mortality after operations for hernia generally depends upon causes entirely independent of the operation, that opening the sac does not increase the danger, and that, therefore, as presenting considerable advantages, this practice ought, as a general rule, to be adopted. He observes:]

It has been urged by the advocates of Petit's operation, that by its adoption, we avoid the following dangers, viz.:

Peritoneal inflammation, consequent upon the exposure of an in-

flamed or strangulated portion of the bowel, which, according to Key, is the cause of death in the majority of cases.

Hemorrhage into the cavity of the abdomen, should a vessel be wounded.

Risk of wounding the intestine.

Rupturing the bowel, by drawing it down from under the stricture, as sometimes arises from an ulcerative process induced in the bowel by the pressure of the edge of the constricting band, which accident cannot attend the division of the stricture on the outside of the sac.

Opening the sac, and thereby laying bare the peritoneal cavity of the abdomen.

Immediate manipulation of parts so important to life; and, according to Gay, inducing unhealthy processes in the external wound.

Exposure of a portion of bowel, possessing feeble powers of resistance to the morbid influence of air, light, &c, giving rise to fatal inflammation and organic changes, not present at the time of operation.

Extreme prostration, speedily followed by death, in cases where great depression of the powers is observed to precede the operation, without any other obvious cause than the exposure of the bowel.

Danger of disorganization, from exposure of contents of hernia bruised by the taxis.

Small collections of pus, at the mouth of the sac, after the operation for opening the sac.

[Mr. Hancock believes that by exaggerating the danger of these circumstances, many have been led to delay the operation beyond the proper period, and to use local and general measures, which do more harm than good. He states that peritonitis is a comparatively rare cause of death after hernia operations, and that there is much less danger incurred by cutting the peritoneum than is generally supposed. Mr. H. says:]

The history of strangulated hernia proves, by every day experience, that the peritoneum may be cut with impunity. I am willing to admit that, if we cut or irritate healthy peritoneum, we may induce peritonitis, although even this does not always occur; but if we cut inflamed peritoneum, the inflammation does not necessarily increase, especially when that inflammation results from some exciting cause, and by our incision we remove that exciting cause. Our incision thus becomes a relief to the patient, whereas, when made in healthy peritoneum, we inflict a violence on the part. I believe, and I am supported in this belief by the observations of Sir C. Bell, that we may cut diseased peritoneum with greater impunity than healthy. The abdominal sections for ovariotomy prove this; the removal of large portions of omentum proves it; the operations for paracentesis abdominis prove it; and the success which attended my case of cœcal disease tends to prove it. Again: is the general peritoneal cavity laid bare by opening the sac? It

must be remembered, that the communication between the sac and abdomen is completely closed by the protruding gut or omentum, and that, even when the intestine is returned, it usually lies so close to the ring, that very little, if any, air can penetrate the abdominal cavity, even if it should be injurious, of which I have great doubts, having had patients who, from wounds, have had the abdominal cavity exposed for a much longer time than would be required for the operation for strangulated hernia, and who have recovered without any urgent symptoms; and, moreover, Key's own observations negative this position, inasmuch as he says, "In tracing the inflammation consequent upon the operation for hernia, it is found to spread from that portion of the bowel that has been strangulated, over the peritoneal surface of the intestines, and not to have its origin from the incision in the sac, although two wounds are usually inflicted upon it. The peritoneum, about the seat of stricture, exhibits fewer signs of acute inflammation than the investment of the bowels"—a conclusive proof that the danger and inflammation result from the violence inflicted on the gut itself, by the strangulation, and that opening the sac, and thereby laying bare the abdominal cavity, have literally nothing to do with the fatal termination; for Key admits that the inflammation does not emanate from the peritoneal sac, although two openings are made in it; also, that the peritoneum, at the seat of stricture, exhibits fewer signs of inflammation than the investment of the bowels, although this portion of peritoneum next to the sac would be the most exposed to the influence of light and air: and, besides, if laying bare the abdominal cavity exerted any influence, it is right to assume that the peritonitis would be generally diffused over the whole peritoneal surface, and not confined to the constricted intestine.

Mr. Lawrence and Mr. South differ from Monro and Key as to the influence of air, light, and handling, in producing fatal inflammation and organic changes not present at the time of operation, and assign such pathological changes to the pressure of the stricture which affects the parts, not like the slight violence of the operation, for a few minutes only, but uninterruptedly for hours.

Cases certainly occur "in which the patient appears to be doing well after the operation, but after the lapse of two or three days the powers begin to sink; the abdomen, though not very tense, is uneasy under pressure, the pulse small and quick, and the tongue becomes coated; this condition is perhaps protracted for several days, and the patient at length dies. A post mortem inspection discovers the cause of death in the dark colour and lacerable condition of the strangulated portion of bowel, and the vascular state of the surrounding parts;" and we are told, that "this unexpected termination usually takes place in patients of enfeebled constitution, whose powers are unequal to the restoration of the healthy circulation in the strangulated bowel after its release from the stricture, and in whom, therefore, a slight degree of inflammation gradually ends in the extinction of its vitality; at the period of operation, the intestine, when exposed, presents none of the usual indications of

present or approaching gangrene; no infiltration of its tissues; no discoloration beyond that which retarded circulation in a healthy bowel produces: no lack of peritoneal lustre, and no lacerability of texture. It in no point appears to differ from those cases of strangulation in which an early operation is had recourse to before severe symptoms come on, and in which a favourable prognosis is verified by a rapid convalescence. Exposure of a portion of bowel possessing such feeble powers of resistance to morbid influence cannot but tend to increase, probably to excite, a disposition to inflammation, which, though low in degree, is sufficient to destroy its vitality, and it may therefore be fairly regarded as the main agent in the production of gangrene."

I quite agree as to the occurrence of such cases, but certainly not with the deductions advanced. If the gangrene of the gut were really due to the cause assigned, would not such cause and effect more quickly and uninterruptedly succeed each other?—would the patient appear to be doing well after the operation?—would the evacuations be free and natural, and the pain and sickness cease, for the first two or three days? In some instances, where the patient has been almost collapsed when the gut has been so much injured as to render it doubtful whether it any longer preserved its vitality, as in Sir A. Cooper's case, such almost lifeless intestine has been left exposed in the wound, the stricture being divided, and the exposure, so far from destroying the patient, has been followed by complete success, the gut recovering its almost suspended vitality, and the patient ultimately doing well. The fatal terminations may, I believe, be much more correctly ascribed to the purgative medicines administered, both before and after the operation; the gut has been subjected to violence, from the constriction, almost too great for its power of restoration, but in consequence of the continued irritation kept up by the purgative medicines, the feeble efforts at recovery are overcome, and mortification is the result.

Lancet, July 21, 1849, *p.* 62.

[Mr. Hancock gives a letter which he received from Sir John Fife, of Newcastle-on-Tyne, in corroboration of some of his opinions. Sir John says:]

"When a student, I saw three cases of hernia, in which the ordinary operation was apparently well performed, but each of these three cases terminated fatally, and in each a post mortem examination demonstrated intestinal obstruction from adhesions, agglutinating opposite convolutions, or otherwise narrowing the calibre of the intestine. These cases made a strong impression upon my mind, and since that time I have operated many times a year for more than thirty years, and I have never considered my operation satisfactory without passing my finger through the peritoneum into the abdomen, and ascertaining that the intestine was free within.

In 1831, while accompanied by Dr. Foster, surgeon to the 4th

Dragoon Guards, Mr. Parr, and many other gentlemen who came to the Newcastle Eye Infirmary to see some cataracts extracted, I was summoned to operate on a poor woman at a short distance, who had been in danger from strangulated femoral hernia for three days; fortunately those gentlemen accompanied me. The operation was apparently completed in a few minutes; but on passing my finger into the abdomen to assure myself that the intestine was free to perform its functions; I felt it bound down to the sheath of the iliac artery by a strong adhesion. My companions then examined, and were satisfied of the fact. They were aware of the necessity of dividing the adhesion, and also of the extreme hazard of the step. The finger nail was tried in vain; at last I introduced a bistoury with great caution. The intestine was fixed, but the gush of blood that followed was such as to leave no doubt of the catastrophe that had occurred. I pressed my left forefinger on the iliac through the wound; with the other hand I enlarged the external wound upwards and outwards, and was soon enabled to pass a ligature with a bent probe under the artery to secure it. This poor woman never had a bad symptom afterwards. The circulation gradually returned to the limb, and the ligature came away about the twentieth day.

This case shows two important facts, which demonstrate the correctness of your views.

1st. That unless the peritoneum be freely opened, and the abdomen examined for some little space within the stricture, an operation for hernia cannot always be considered complete.

2nd. That the external iliac may be cut, and tied through the peritoneum, and the ligature may remain in the peritoneum twenty days without inducing peritonitis. The woman died of cholera some years afterwards, when Mr. Parr made an autopsy, and dissected out the ligatured portion of the external iliac."

[Mr. Hancock proceeds to say:]

I now pass on to the consideration of the statistics of the two modes of operation, as furnished in a late work on femoral hernia, and would here express my dissent from the position assumed by Mr. Key, that notwithstanding "the danger of the operation is by no means the same under all the circumstance attending strangulated hernia; that when the operation is performed early, before untoward symptoms have supervened, it is usually attended with a fair prospect of success. In estimating the risk of an operation, we must not separate the favourable from the unfavourable cases; we should form our judgment upon the nature and the issue of the aggregate cases that present themselves to surgeons in the ordinary course of practice.

In forming an estimate of the relative value of two operations for the same disease, we should base our calculations upon operations performed upon cases as nearly similar as possible, and not confine our operation to favourable cases, employing the other indiscriminately, and then cast up the successful and unsuccessful terminations without reference to collateral circumstances.

In Table 1, contained in the work already alluded to, we find the following analysis of 198 cases, showing, according to the author, the relative amount of mortality attending each mode of proceeding; and I am bound to acknowledge that the proportion of recoveries, according to this table, appears greatly in favour of Petit's operation, being fifty-two deaths to seventy-three recoveries, where the sac has been opened, and only thirteen deaths to sixty recoveries, where it has not.

Whence obtained.	Total cases.	Sac opened	Recovered.	Died.	Sac not opened	Recovered.	Died
Miscellaneous ...	57	48	23	25	9	7	2
Howship	8	6	3	3	2	—	2
Poland, Report of Guy's Hospital	17	12	5	7	5	3	2
Mr. Luke	82	25	17	8	57	50	7
St. George's	34	34	25	9	—	—	—
	198	125	73	52	73	60	13

I will take the cases of Howship, Poland, Luke, and Hewett, (of the miscellaneous, I have no means of judging,) from the above calculation, and endeavour to show the degree of value which we ought to attach to the statistics advanced. Mr. Howship's operations are eight in number, six in which the sac was opened, two according to Petit's mode, both of which died. Of the six in which the sac was opened, three recovered, three died; one from ossification of the aorta and larger vessels, one from ulceration and bursting of the intestines, and the other, evidently, from purgatives and tobacco administered.

Again, take Poland's recorded cases of Guy's Hospital, seventeen in number, of which seven died, the sac having been opened. Of these seven, one had ruptured the intestines in his endeavours to return it before the operation; two had gangrenous intestine; two, gangrenous omentum; one, the parts had so nearly lost vitality, that the wound was not closed, while the last proceeded favourably until the third day, when, having had purgatives (enemata and medicine) daily, since the operation, violent purging ensued, followed by peritonitis, which, after the patient was leeched, bled, &c., destroyed him.

The most ardent supporters of Petit's operation will scarcely assert that opening the sac had anything to do with the death of the patients in these cases, particularly when it is remembered that purgatives, medicines, and ejections were administered in all, both before and immediately after the operation. And it may be asked, would the condition of these patients have been at all improved, had the sac not been opened, and consequently seven, if not the whole, of these cases, should be left out of the calculation. Again, with respect to Mr. Luke's cases, which appear so greatly to favour the modern operation. These are given at eighty-two, in twenty-

five of which the sac was opened, resulting in seventeen successful and eight unsuccessful terminations, whilst of the remaining fifty-seven in which the sac was not opened, fifty recovered, and only seven died But Mr. Luke, with his usual candour, throws a very different light on the matter. He says, "Between 1831 and 1841 I have attempted the performance of Petit's operation, in 84 cases. Of this number, the operation was completed successfully without opening the sac in fifty-nine. In twenty-five, it was necessary to open the sac to effect reduction, the opening generally varying from half to three quarters of an inch. With respect to the mortality of the fifty-nine, seven died; of the twenty-five, eight died. He adds, however, although it may be expedient thus to state in a general summary the results of all cases submitted to Petit's operation; yet, for the purpose of satisfactory comparison, those cases which were selected from numerous others, and which, therefore, may be presumed to yield results more satisfactory, should be excluded, as also should those cases be, which, from their nature, could not be expected to be benefited by any operation of any description, or even by the taxis, could it have been successfully performed." For the above reason, he excludes twenty-six cases selected between 1831 and 1841, and also four others, three of which were moribund at the time of the operation, and the other had recovered from the operation, but died six weeks after from ulceration of the bowel, so that subtracting these thirty cases, fifty-four remain. Of the fifty-four the sac was opened in twenty-one, of which three died; and not opened in thirty-three, of which two died; so that, considering the class of cases requiring the opening the sac, the rate of mortality does not appear to be so much greater in that mode than where the sac was not opened. I cannot agree with Mr. Luke in considering that "the necessity to open the sac implies neither a greater degree of constriction of the hernial contents, nor a consequently greater severity of case from organic lesion, than in instances in which no such necessity occurs." For if there be greater danger in handling the gut itself, than through the sac, surely there must be greater danger where constriction is made directly on the gut by the neck of the sac, than where such constriction is caused by the tissues around the sac, and so exerts its influence upon the neck of the sac, and through it upon the intestines. And this is borne out by the fact that the mortality was greatest in Luke's cases, in inguinal hernia, where stricture at the neck of the sac most frequently occurs. However, it must be admitted that Mr. Luke has been most successful; fortunate alike in the result of his operations, and in the class of cases submitted to his knife, for few surgeons, if any, can boast, like him, of having operated upon fifty-four unselected cases of hernia without meeting with a single instance of complication.

Lastly, take Hewett's cases, thirty-four in number, of which nine died, all having the sac opened. Of these nine there was sloughy intestine in one instance combined with pneumonia; in one, sloughy omentum; in one, portions of sloughy placenta in the uterus,

found after death; in one, disease of the brain; in two, the intestine and omentum were left in the sac, in consequence of adhesions; and in one only was there mere peritonitis, combined with intestine of a dark mahogany colour, but neither thickened nor softened.

I need not go further into these details; I trust that I have entered sufficiently into the subject to prove that the mortality, even according to Mr. Gay's statistics, is not due to opening the sac, but to circumstances totally unconnected with the operation.

We have here the account of 126 cases, in 86 of which the sac was opened. After deducting the 15 which must have proved fatal, whatever operation had been performed, 71 fair average cases in which the sac was opened remain. Of these eight terminated fatally, or one in nine, whilst on the other hand, of the forty in which the sac was not opened, after deducting one in which the aorta was ossified, we have 39, of which five terminated unsuccessfully, or one in seven and a half.

Through the kindness of Mr. Hamilton and Mr. Puleston I have been enabled to obtain the particulars of 548 cases of strangulated hernia, collected from various sources. Of these the sac was opened in 474, the operation proving successful in 307, unsuccessful in 167. In three the gut was injured in dividing adhesions; sixty-one proved fatal without any complications, and of the remaining 103, in thirty-three the gut was gangrenous; in thirteen it was gangrenous, and had burst; in thirteen the gut was perforated by ulceration; in one the patient had burst; the intestine in his endeavours to return it before the operation; in six, the intestine was adherent to the internal ring; in seven, adherent to the sac; in eight, gangrenous omentum; in seven, adventitious bands and adhesions together; in two, double sacs; in two, sacs contained pus; in one, colon was twisted, and could not be returned; in one, Petit's operation was first tried and failed; in one, patient delirious, in *articulo mortis;* in one, sloughing placenta found in the uterus after death; in one, disease of the brain; in one, ossification of the aorta; in one, encysted hernia of tunica vaginalis, unrelieved; in one, pneumonia, after twenty-three days; in one, constricted intestine at point of stricture; the rectum severely ulcerated from very hot enemata; in one, patient had been operated upon three times, and in another, the patient had taken several doses of croton oil, turpentine, &c., before the operation; immediately the gut was liberated, the bowels began to act, and continued to do so, almost without intermission, until she died, twelve hours after.

It should be stated, that the condition of parts here given was in almost every instance found at the time of operation, and not merely observed after death; it must consequently be admitted, that the above 103 cases would have equally terminated fatally had Petit's operation been performed, consequently the number should be calculated at 307 successful, sixty-four unsuccessful, or one in six.

On the other hand, the sac was not opened in seventy-four. In five, it failed under circumstances which would have equally influ-

enced any operation. I consequently leave those out, and take unsuccessful cases, in one of which the gut was wounded, at seventeen, leaving fifty-two successful cases, or one in four.

Lancet, Sept. 8, 1849, *p.* 263

[Mr. B. Travers, jun., also stated, at a meeting of the Westminster Medical Society, his preference of the ordinary operation. His objection to Petit's operation was, he said, that in some cases the operation was performed without removing the strangulation. He said that,]

If in only one or two cases out of a hundred such a mishap could occur, he thought it sufficient to make the operation in question unjustifiable. He could not, as an operating surgeon, appreciate the fear which existed in the minds of some respecting the division of the peritoneum,—he, on the contrary, regarding such section as beneficial, by relieving the congestion which existed, and removing the fluid which had accumulated. He believed it was a rare occurrence for hernia to be fatal from peritoneal inflammation; and when this did occur, it was usually the result of constitutional causes, and the mischief was found to be situated at parts distant from the seat of the operation. He related the following case:—He was called to a fine young woman with all the symptoms of strangulated femoral hernia. Under the influence of steady pressure, exerted over the tumor for about a quarter of an hour, the hernia was reduced, with an audible noise: it was evidently intestinal. Some mild injections were administered after several hours, and she appeared well for about fourteen hours. At that time all the symptoms of strangulation recurred; and at the end of thirty hours from the first seizure she died. The operation of opening the sac had been unsuccessfully performed during the last few minutes of her life. A piece of adherent and sloughing omentum was found at the bottom of the sac.

[In reply to these remarks, Mr. Gay allowed that the great fatality which followed hernia operations, could by no means be attributed to peritonitis. He believed that death was very frequently caused by the large and deep wounds made in the ordinary operation for hernia, which would be especially dangerous if the individual was suffering from disease of other organs. He said:]

With regard to old-standing disease of the heart, from the intimate sympathy which is known to exist between that organ and the peritoneum, it is of the utmost importance that in persons so affected the sac should be allowed, if possible, to remain entire; and that the external wound should be as small as possible. In the cases examined by Mr. Gay, he had found that where hypertrophy of the heart existed, the wound, in all instances, became erysipelatous and sloughy.

The question is, in a number of cases of strangulated hernia—What is the predetermined plan of relieving the constriction? and

the answer is in favour of those cases in which the sac has not been opened. But Mr. Gay had no preference of the one operation over the other on this account, for it is most probable that the comparatively favourable results of the one system depended really upon something more—viz. the fact that those cases really relieved are by far the most favourable for operation, not in general being so complicated as those in which it becomes necessary to open the sac. Petit's operation is simply the expression of a principle, for which Mr. Gay had endeavoured to obtain the sanction and support of the profession—viz. that of not wounding more tissues, or going deeper with your incisions in a case of strangulated hernia, than is absolutely necessary for the relief of the constricted parts; and especially to avoid interference with the hernial tumor and its envelopes, which are usually in a more or less diseased condition. The lamellar separations of fasciæ in these operations are much to be condemned, and may be dispensed with. They are only made as guides for the operator; but as such they ought not to be continued. Mr. Gay's plan is, if the case appears to be favourable, to divide the stricture first outside the sac; and if that does not allow the parts to be returned by the employment of the most gentle compression by the fingers, then the sac is to be opened, and the cause of detention removed, whatever that may be. Mr. Gay recommends and practises a small incision, sufficiently large to admit the finger through the skin and superficial fascia, on the inner side of the hernial tumor, and in such a position that he is able to reach the seat of stricture, and divide it without interfering with the sac, excepting at the point where it is constricted. Mr. Hancock says, that the contents of the sac should be examined before they are returned, for fear of their being replaced whilst some constricting band of false membrane still keeps up the constriction, is in a condition of sphacelus, or actually ulcerated or cut through; and it has been remarked, that in every case the sac should be opened, for the purpose of ascertaining the state of the contents. This doctrine would obviously, as Mr. Smith has justly remarked, oppose itself to the restoration of the viscera by the employment of the taxis alone, were that possible—a plan which no one would venture to abandon. The previous history of the case is always sufficient to throw some light upon the state of the herniated viscera,—such as the length of period during which hernia has existed; whether it has been kept up by a well-adapted truss, or allowed to remain neglected; whether the patient has had any attacks of peritonitis, previous to the occurrence of strangulation; whether the strangulation be recent, or has existed for several days, &c. Mr. Hancock arrayed Sir Astley Cooper amongst the opponents of Petit's operation. Mr. Gay would remind Mr. Hancock that Sir Astley Cooper had said, with regard to it, that he felt convinced that "this operation will be gradually introduced into general practice, when it has been fairly tried, and found, if performed early, to be free from danger, and attended with no unusual difficulty." In the open operation there is no difficulty in extending the external incision, so as to en-

able the operator to open the sac, if it be found necessary. Such a case occurred in Mr. Gay's practice, two weeks since. A young woman was admitted into the Royal Free Hospital, with a strangulated femoral hernia. The femoral and Gimbernat's ligaments were divided outside the sac, but the contents could not be returned. Mr. Gay then enlarged the external wound, carrying it across the neck of the tumor; opened the sac; discovered a portion of omentum, and a knuckle of intestine twisted on itself. The neck of the sac was cut, the intestine relieved, but the omentum was found to be adherent to the walls of the abdominal cavity in such a manner that it could not be released. The woman has recovered, and the omentum serves as a natural plug against any future descent.

Medical Gazette, June 1, 1849, *p.* 959.

[Mr. Cock, Surgeon to Guy's Hospital, has reported in the last volume of the Guy's Hospital Reports, several most interesting cases of hernia, appended to which he makes some very pertinent remarks; and, as in one case especially, these remarks refer to the division of the stricture external to the sac, we will give them in this place. The first case was one of omental hernia, respecting which Mr. Cock says:]

We are frequently called to a patient suffering from a recently-descended hernia, consisting of intestine and omentum: we succeed in returning the former, while the latter remains. The urgent symptoms are relieved, and we endeavour to persuade ourselves and our patient that the retention of omentum is a matter of no consequence: we even indulge in the fallacy that it will act as a plug to the ring and prevent the future protrusion of bowel. But perhaps we are too much inclined to forget, to overlook, or to slur over the fact, that from that moment the individual is placed in a state of positive inconvenience and possible danger. A portion of irreducible omentum, so far from forming a plug to the abdominal outlet, offers actual facilities for the future descent of intestine, at the same time that it, in great measure, renders abortive the usual means employed to prevent the recurrence of a hernia. It also renders the diagnosis and the treatment of every fresh protrusion more obscure, uncertain, and dangerous. In the above case there is but little doubt that, could the omentum have been returned in the first instance, any subsequent return of hernia might have been prevented by the judicious application of a truss, and the fatal result might have been staved off, if not altogether avoided.

The necessity of abandoning a portion of omentum unreduced in a sac must always be a matter of dissatisfaction to the surgeon, as being a half curative plan of treatment, leaving his patient in a condition of anxiety and danger. The extreme difficulty of adjusting a cupped truss, or indeed any truss which shall be tolerated by the patient, and secure immunity from a future descent of bowel; the additional danger and difficulty which attends such a descent, in consequence of the sac being already partly occupied;

to say nothing of the occasional mischief produced by bands of omentum extending from the abdomen through the rings, and every now and then giving rise to fatal and irremediable internal obstruction, several cases of which have come under my own observation;—all these circumstances should, I think, induce us to adopt every means to empty a hernial sac of the whole of its contents, even to the extent of recommending an operation for that purpose. In many, perhaps in most instances of irreducible omental hernia, the patient is unable to sacrifice the time, or unwilling to subject himself to the discipline necessary for starving out a portion of protruded omentum, and rendering it capable of being returned into the abdomen; and it may then become a question whether the safety of the individual would not be less compromised by the prompt and decisive treatment of operation, than by the adoption of more uncertain palliative measures.

The success which generally attends an operation for hernia when the peritoneum has not been opened, and the ease with which the contents of a sac are frequently returned after the stricture has been freely divided externally, are such as to deprive the extreme measure of half its terrors, and enable us to recommend it with greater confidence to our patients.

I have no intention of entering further into the treatment of irreducible omental hernia; but as it happens in many, perhaps in most instances, that a patient will neither submit to operation nor coercive measures requiring rest, recumbent posture, and discipline as regards diet and medicine—as, in fact, they generally insist on going about their ordinary avocations—I perhaps may be allowed to mention the plan which I have generally found most effective in supporting the rupture, keeping it within its due limits, and preventing a further descent. Much disappointment will be found in the adaptation of trusses, which generally serve only to worry the patient, and fail to secure the object desired. I have been for some time in the habit of placing a pad, closely stuffed with wool, over the tumour, and securing it in its position by a roller of caoutchouc webbing, a few turns of which are passed round the loins and the thigh, so as to form a figure of 8 crossed over the groin. This forms by far the most efficient mode of support that I am acquainted with, and enables the patient to use his limbs and go through any kind of exercise with great ease and freedom. It is perhaps more applicable to femoral than to inguinal hernia, but, with a little modification, may be adapted to either case.

In several instances, after the apparatus has been worn for some weeks, I have been much pleased to find the tumour sensibly reduced in size; and occasionally have been still farther gratified at being enabled, with a little manipulation, to empty the sac of the whole of its contents.

[Mr. Cock's next case is chiefly to illustrate a favourite practice of his, that of keeping the bowels very quiet for several days after the operation. He says:]

I have always found calomel a most essential and invaluable adjunct to the opium, as it appears to maintain the healthy functions of the skin, liver, and kidneys, and renders the subsequent unloading of the bowels easy of attainment, without the exhibition of drastic aperients.

This is one of the few cases in which, after dividing the stricture external to the peritoneum, I have been obliged to open the sac before I could effect a reduction of its contents. The facility with which, in almost every instance, the bowel may be returned, after making an external division of the stricture, is now a fact too notorious to admit of dispute; and I should hardly have alluded to the subject, did I not find it recorded by a late surgeon, of considerable repute, when alluding to this operation: "In nine cases out of ten, he (the surgeon) will fail, and he is much more likely to succeed in pushing back sac and all." * * * "It is rarely possible to effect the object, although there is no harm done by trying it in some cases; but in others, constituting the vast majority, the attempt must prove very unsafe," &c., &c.

As experience has proved that the object may be effected nine times out of ten by dividing the stricture external to the sac; that with moderate care there can be no danger of pushing back sac and all; that the process is less severe, and the result more successful than by the old method; it is to be regretted that so hasty and erroneous an assertion should have been recorded in a book on practical surgery.

One of the points of interest in this case is the state of the intestine, which, from the duration and the severity of the strangulation, had undergone so great a change, that although not actually gangrenous, some doubt might have been entertained of the propriety of returning it into the abdomen. I here followed a principle of which experience has taught me the value—that the safest place for an intestine not actually perforated, but which is in a doubtful condition, and of which any portion is likely to lose its vitality, or to become opened by ulceration, is the abdomen. By restoring the gut to the peritoneal cavity, we afford a fair opportunity for the mending of any subsequent breach in its coats and for the preservation of the integrity of its canal, by the adhesions which it may contract to the opposite serous surfaces. I believe the patient will thus have a better chance of recovery than that afforded by trusting to the forlorn hope of leaving the bowel in the hernial sac. I some time ago operated on a young woman, in her fifth month of pregnancy, and returned the bowel without opening the sac. She went on favourably for a week, when she miscarried, and died of hemorrhage. On examining the portion of intestine which had been strangulated, I found a piece about the size of a shilling, including the whole thickness of the gut, had been completely separated, although not yet thrown off, by a process of sloughing. The breach, however, was accurately patched by adhesions to the neighbouring intestine, and no reasonable doubt could be entertained, but

that, had she lived, the reparative process here exerted by nature would have been perfectly successful.

One of the strongest objections which have been made to the operation of dividing the stricture external to the sac, is the probability of returning gangrenous intestine or omentum into the abdomen; and the case which I have related may seem an example in point. I conceive that in practice the objection will hardly hold good; for I am led to believe that, in most instances, when the contents of the sac are not in a state to be returned, the very circumstance of their unfitness, the absence of fluid, the partial adhesions, the loss of polish, will prevent their reduction, except by the use of such force as the previous circumstances and symptoms would inform us we are not justified in exerting.

[The next case was one of injured hernia in a child, in which case he divided the stricture external to the sac. Mr. Cock then says:]

Of all forms of hernia requiring the use of the knife, there is none which the surgeon approaches with more unwillingness, or encounters with greater dread, than the large, tense, scrotal tumour, containing a volume of omentum and intestine. No sooner is the sac opened than the contents are forcibly protruded; and after the division of the neck, the surgeon is foiled and embarrassed in his attempts to return the intestine and omentum by succesive fresh descents from the abdomen. The annals of surgery furnish many fatal cases of this kind where death from peritonitis followed a long tedious operation, during which large portions of bowel suffered irrecoverable injury from exposure and manipulation. When a child is the subject of such an operation, the difficulties are much enhanced by the greater preponderance and extent of muscular fibre in the abdominal walls, and the constant struggling and effort which it is impossible to control. All these difficulties and dangers are avoided when the reduction can be effected without opening the sac. About two years ago I operated on a gentleman, with a very large scrotal tumour, and succeeded in returning the whole of the contents of the sac by dividing the stricture external to the peritoneum. I have been equally successful in another similar case; and feel convinced that the operation of dividing the stricture external to the sac, as originally recommended by Petit, and of late revived and extensively practised by Mr. Luke, the late Mr. Key, and other surgeons, constitutes one of the most valuable improvements of modern surgery.

Guy's Hospital Reports, Vol. VI., part 2, Oct. 1849, *p.* 309.

77.—*Case of Obstruction of the Bowels for which Amussat's Operation was performed.*—By D. W. Crompton, Esq., Surgeon to the Birmingham General Hospital.—[Mr. Crompton's patient was a man 36 years of age, who had been obstinately constipated for eleven days before the performance of the operation, notwithstanding the employment of all proper means. It was ascertained by the use

of bougies, &c., that there was obstruction about the situation of the sigmoid flexure. The patient had stercoraceous vomiting, the abomen was much distended, and collapse came on shortly before the operation was performed. Mr. Crompton tells us:]

I began M. Amussat's operation by making an incision in the left lumbar region, about an inch above the crest of the ileum, a little nearer to the spine than was necessary, and carried it outwards through the skin and fat to the extent of four or five inches, laying bare the fibres of the latissimus dorsi, which I divided, and then the quadratus lumborum, till I came down to the anterior fascia, which was carefully cut through; and then a quantity of fat appeared, which I removed by pulling it and cutting it, till a dark bluish membrane came in sight. We were uncertain as to what it might be, as some such membrane I had seen in Mr. Field's case, which proved to be only a membrane covering more fat; however, I thought it prudent to pass an armed curved needle through it, lest it should be the bowel we were in search of, and such it proved to be; for, as soon as the needle had penetrated it, a quantity of gas and liquid fæces spirted out. Having secured the bowel by two sutures to the sides of the wound, I made a longitudinal incision between the upper and lower suture, which allowed a basinful or two of fæces to escape; and the man was carried to bed and laid on his back. He never expressed the least pain,—partly, I think, from the depressed state in which he was. Brandy and beef tea were given him, and hot flannels applied, but in five hours he died.

On a post mortem examination, we found the distension of the abdomen gone. On opening the abdominal parietes, the small intestines presented a slight blush, and at the contiguous margins there was the red line of commencing peritonitis, with here and there a very fine layer of lymph. On removing the small intestines altogether, the colon was well seen,—distended, but not impacted; the descending colon looked as if drawn backwards at an angle of about ten or eleven inches above the sigmoid flexure. There was no effusion of any kind, or appearance of inflammation about the bowel or wound. The descending colon, with the wound on the parietes still adherent, was removed with the rest of the bowel to the anus, and slit up, when it was found that at eleven inches from the anus there existed a firm contraction of the peritoneal coat, as if a whip-cord ligature had been tied round it. On the mucous surface there was an ulceration, with thickening to about the third of an inch; and still lying upon the ulcerated surface were a number of currant seeds. The opening made into the bowel by the operation was just ten inches above the strictured part.

The other organs of the body (the head not being examined) were healthy.

Medical Gazette, July 20, 1849, *p.* 107.

78.—*Operation for Artificial Anus.*—By Professor FERGUSSON.—[In this case there was an opening into the rectum, the result of suppuration which had been connected with morbus coxarius.]

The opening existed on the right side between the sacrum and the back part of the ilium; it was two inches in length, and the gut could be seen protruding almost through it,—more especially when the patient made any violent respiratory effort. An orifice existed in the back part of the gut, in which the finger could be placed, and through which the whole of the fæcal matter was evacuated. A bougie could be passed from the anus through this false opening. No fæces ever came *per anum*, and the artificial anus had existed for four years. The disease in the hip-joint had become cured, anchylosis having taken place.

Mr. Fergusson stated, that the patient had been sent up to him more as a curiosity, than from any idea he could do anything for him. He, however, thought that he might be able to close the opening, and he should endeavour to-day to cover in a portion of it; and if he succeeded in this, he should finish the remainder at a future day.

He then operated in the following manner:—The soft parts, which were lax and healthy, were dissected upon each edge of the opening, so as to permit them to be brought into contact. The edges of the wound were then well pared, and two hare-lip needles were introduced, by means of which the lower half of the aperture was well closed without any tension being exerted on the parts.

Medical Times, Aug. 4, 1849, *p.* 92.

79.—*On the Treatment of Fissures of the Anus.*—By W. J. ANDERSON, Esq., London.—[The ordinary mode of treating this disease is by section of the sphincter ani. Mr. Anderson recommends this operation to be performed in a manner different from that which is usually adopted. He says]

The operation adopted by this great surgeon, (Boyer) and which is now generally performed, consisted in introducing the finger into the rectum, and upon it a probe-pointed bistoury, with which he cut through, "at a single stroke, the intestinal membrane, the sphincter, cellular tissue, and tegument;" thus producing a good-sized open wound. Now every surgeon must be aware that it is perfectly possible for this to be followed by a considerable amount of inflammation, and that if suppuration does occur in the loose cellular tissue around the rectum, it is an exceedingly awkward affair. This danger may be almost entirely avoided, and the operation much modified, by the following plan, which I learned from the late Professor Blandin. The instrument employed for this operation consists of a sharp-pointed straight blade, with a flat sliding guard upon one side of it. This guard is rounded at the extremity, oval externally, and flat upon the surface, lying on the

blade. It is compressed against the side of the knife by means of a spring, and retained in its position by a slightly projecting pin, sliding in a groove upon the blade. The whole or a portion of the guard can be retracted within the handle by a button attached to it for that purpose. The forefinger of the left hand is introduced into the rectum, and a small opening is made at the verge of the anus with the point of the knife. The guard is then protruded beyond the point, and the instrument carefully introduced through the wound into the sub-mucous cellular tissue between the mucous membrane and the sphincter ani. The cutting edge is then turned towards the muscle, the guard retracted, and the muscle cut through as the blade is withdrawn. If any doubt remains as to whether the muscle is completely divided or not, the instrument (guarded as before) may be re-introduced, and a second cut made in a similar manner, exactly upon the first. But if Boyer could cut through all the tissues, in his operation, at one stroke, surely we may do it also, especially when we have not the intestinal membrane and tegument to deal with. The small external wound should then be closed, and the bowels kept in a lax state by means of the confection of senna, given in sufficient quantities to render the motions moist; but purging should be carefully avoided. It may be as well to apply some unctuous substance locally, to prevent any irritation from the fæculent matter.

We have here, at any rate, an operation far less repulsive and formidable to the feelings of the patient, and certainly, in a practical point of view, attended with much less risk. The relief is as instantaneous as in the other operation; but no large open wound is left in the intestine; defecation can, therefore, be performed without the same amount of irritation; and the cure is materially expedited, inasmuch as the parts are not kept in a state of disunion longer than is necessary for its completion. Of course the same preliminary treatment, with regard to unloading the bowels, is necessary in this as in the other operation; and those excellent remarks by Sir Benjamin Brodie, concerning the impropriety of cutting directly forwards in the female, or directly backwards in either sex, are equally applicable to both methods of operating.

Lancet, Sept. 15, 1849, *p.* 291.

80.—*Cases of Hemorrhoids, treated by Nitric Acid.*—By Dr. R. T. Massy, Physician to the Exeter Dispensary.—Pure nitric acid performs most rapid and wonderful cures in bleeding piles, whether external or internal, and is a safe and easy remedy in its application. Since Mr. Cusack, of Madam Stephen's Hospital in Dublin, first introduced it into practice, I have frequently recommended surgeons to try it before using the ligature or knife. I can assert, from some experience in using it myself, and from the writings and remarks of others, that we have not a better or safer application to *cure* hemorrhoids.

One gentleman, who consulted me in May, 1847, described his

piles as hanging about his legs. I shall give the notes from my medical case-book of record:—

A. B., aged 30, a farmer, of fair complexion, stature 5 feet 6 inches, countenance anæmic, has been an invalid for some years with bleeding piles, which came on gradually; and to them he has a family predisposition. Present state of the complaint:—Has got a large vascular tumour of the rectum protruding at the verge of the anus, which bleeds half a pint of black blood immediately after stool. The tumour has two bleeding surfaces, with an excoriation of almost an inch square.

His bowels are moved at ten o'clock, p.m. every night by taking at noon each day (which is an hour before his luncheon), a wine-glassful of the following aperient mixture:—

℞. Infusi sennæ, ℥vi.; sulph. magnesiæ, ʒiv.; carb. magnesiæ, ʒiss. M. ft. mist.

One night after stool, as he lay on his side on a mattress, I applied, with the feather of a quill, nitric acid to the bleeding surface; and when the part turned white, I laid on a pledget of lint, covered with olive oil, and then returned the gut.

Complained of slight darting pains during the night.

Got up on the second day; made an examination on the twelfth day; the tumour is greatly reduced in size, and bleeds very little now; applied the nitric acid as before.

Remained in bed for a day; felt a degree of tightness during defecation, in the region of the anus.

24th day.—Applied the nitric acid again to contract a small remaining portion of the tumour.

36th day.—Is now cured, can ride and drive, and walk about during the whole day; feels a new man.

I have touched with the acid a bleeding surface three inches up the rectum, with the same benefit.

A. C.—This man lost, at each stool, a large quantity of blood; he was quite emaciated and worn out with pains in the back, and down the thighs.

On looking up the rectum I discovered an oval bleeding surface, its greatest diameter being more than an inch; it was soft, vascular, and elevated, and about three inches from the verge of the anus.

Two applications of the acid cured this patient.

A. D., aged 27; this gentleman had external blind piles; two were visible at the verge of the anus, feeling tense, and looking as large as a hazel nut.

Touched them with nitric acid. On the fourth day they were quite contracted, and drawn up.

It is now more than twelve months since he was cured, and he has had no return of them since.

Persons having hemorrhoids should always be advised to defecate just before going to bed; this can be easily arranged by giving a dinner pill,—say three grains of the pilulæ aloës dilutæ, with one grain of gum mastic.

As this pill mass is not given in the Pharmacopœia, I may state its composition:—Pilulæ aloës dilutæ, *Jephson.*

℞. Aloës bbs., saponis hispan., ext. glycyrrh., theriacæ, sing., partes æquales; solve in aquâ; cola spissa leni calore.

Although I always look to the state of the liver, and give a blue pill at bed-time, and a saline aperient early on the following morning, that the bile may be removed, and the bowels cleared out before using the acid, yet I look on many cases as local, and by no means connected with the portal circulation.

I am in the habit of desiring the patient, after I have used the acid, to lie on a sofa, or to remain in bed for the day; notwithstanding this injunction, I have now and then met one, on the sofa-day, at a dinner-party, or walking about town, the pain from pure nitric acid is so slight, causing little or no inconvenience.

Medical Times, June 30, 1849, *p.* 674.

81.—*On the Treatment of External Hemorrhoids by Potassa Fusa.*—By Dr. F. C. Jones, Surgeon to the South London Dispensary.—[Dr. Jones states that within twelve months he has treated between sixty and seventy cases of external piles with undeviating success, by the means which he here describes. He says:]

Observing that in all cases of spontaneous cure of external hemorrhoids, they primarily slough, I was led to think that if nature obliterates the vessels by sloughing it is feasible that artificial means might induce the same remedial action.

In turning over in my mind the action of various remedies likely to produce this condition, it struck me forcibly that potassa fusa was precisely the agent required. I forthwith determined to apply it to the first case that came under my care, and the results were, that upon the first application the patient complained of a burning sensation, which passed off in the course of half an hour; at the end of four days the parts were considerably diminished in size, and by brushing a piece of lint quickly over the part I removed the superficial slough, and again applied the potassa fusa; the same treatment was followed at the interval of four days, at the end of which time they had entirely disappeared, leaving only a slight sore, which healed within a week.

Lancet, Sept. 8, 1849, *p.* 277.

82.—*New Instrument for Operating on Hemorrhoids.*—By M. Amussat.—M. Amussat has invented a new instrument for the eradication of these troublesome companions. It is a small forceps, somewhat like a dissecting forceps. The points, or rather the branches, for one-third of their length, are split, and receive two small hollow cylinders, which are charged with Vienna caustic at the moment of operation. The branches of the forceps are brought together by a screw, and the base of the pile attached by constriction and cauterisation conjointly. When the instrument has acted for some time,

a jet of cold acidulated water is directed on the part, to remove the superfluous caustic and allay the sensation of burning. M. Amussat has already applied his instrument in practice with complete success.

Medical Times, July 14, 1849, *p.* 28.

83.—*Salivation Treated by Nitrate of Silver.*—By — Kirby, Esq., —In a late number of the Medical Press M. Bouchacourt is reported to have used successfully a solution of nitrate of silver in a case of profuse salivation, in which various gargles and sinapsims, &c., had been employed without benefit. I have had much experience of this remedy, having used it for many years in cases of sudden and inordinate salivation. It rapidly arrests stomatitis and the attendant profuse discharge, provided it be applied of proper strength, and with sufficient frequency. Of course the teeth become deeply discoloured, but these stains being removable by means which shall be noticed, temporary disfigurement is not to be urged as an objection to a free and general employment of so controlling a remedy.

A case selected as an example.—Mr. G., under treatment for bronchitis, with engorgement of the liver and heart, was unexpectedly seized with violent salivation and distracting pain of his gums, which were unusually swollen. I ordered the following solution:—

℞. Nitratis argenti drachmam. Aquæ distillatæ unciam. Sol.

This solution was freely applied three or four times daily, with a hair pencil, by my friend Mr. Collins. The heat and smarting which ensued upon each application quickly subsided, and was followed by a period of much ease. In three days the patient's mouth was well.

Until now he had no idea of the condition of his teeth, of whose shape and colour he was always rather vain. His annoyance and dissatisfaction at the "ruinous" change of these adornments was displayed in terms of no equivocal import; his chagrin, however, was soon allayed as he saw them brighten under the use of a solution of hydriodate of potash, in the proportion of two drachms to an ounce of distilled water; which solution was frequently applied by a brush. He now left for the country, and continued this treatment. When I saw him on his next return to town, his teeth had nearly resumed their former attractive lustre; I say nearly, for although darkness had wholly disappeared, yet a slight yellowish tinge was perhaps observable.

Mr. Collins, who has been engaged in some experiments as to the means by which stains from the nitrate of silver may be most effectually removed, has favoured me with the following report:—"I applied a solution of nitrate of silver, in the proportion of a drachm to an ounce of distilled water, to two extracted teeth for some days, until a deep blackness was produced. I treated the one by a solution of the cyanide of potash, in the proportion of one part of the salt to two of distilled water; the other I treated by a

solution of hydriodate of potash, in the proportion of four grains to ten minims of distilled water; and the hyposulphite of potash in the proportion of one part of the salt to two of distilled water. The latter plan succeeded very well, but the former produced a much better effect, more thoroughly removing all traces of discoloration, after being patiently and carefully applied by means of a soft brush.

Dublin Medical Press, Aug. 1, 1849, *p.* 70.

[Mr. COLLINS, in another number of the Dublin Medical Press gives the following further information on the subject:]

If a solution of cyanide of potassium be added to a solution of nitrate of silver, a whitish precipitate of cyanide of silver immediately occurs. This precipitate is perfectly soluble in an excess of the cyanide of potassium, forming a colourless solution.

Black stains produced by nitrate of silver on the skin, nails, teeth, and on linen, &c., brushed over with a solution of cyanide of potassium (eight or ten grains of the salt to ℈j of distilled water) are similarly acted on. They will disappear after one or two applications if at all superficial; if even deeply seated in the textures, they may require several.

The cyanide of potassium is a cheap salt, and may be had at the Apothecaries' Hall, or from any respectable druggist. The solution should be made fresh, as it is liable to spontaneous decomposition.

Dublin Medical Press, Aug. 8, 1849, *p.* 83.

URINARY ORGANS.

84.—ON THE PATHOLOGY AND SURGERY OF URINARY CONCRETIONS.

By B. B. COOPER, Esq., F.R.S, Surgeon to Guy's Hospital, &c.

[One of the most valuable papers recently published on the above subject is by Mr. B. Cooper, from which we shall take the liberty to draw largely, on account of its great value, and with the object of diffusing its usefulness as much as possible. After some preliminary remarks, Mr. Cooper says:]

From the circumstance, that all the constituents of the urine are derived directly from the blood, and that all its ultimate elements are found in that fluid, it follows, that any defect in the function of those organs by which the food is converted into blood, causes an immediate change in the character of the urine, which is consequently found to contain either an excess or deficiency of some of those substances essential to its normal condition. It is evident, therefore, that healthy urine can only be secreted from healthy

blood; and it is probably owing to the influence exercised upon the urine by the changes produced in the blood by interruptions to the processes of assimilation, that indigestion proves the most prolific cause of those peculiar conditions of the urine in which there exists a tendency to the deposition of one or other of its constituents.

The characteristic constituents of the urine, viz. uric acid and urea, contain a considerable quantity of nitrogen, and are probably produced from the nitrogenized constituents of the body by the process termed "metamorphosis of tissue." The principal function of the kidneys seems, therefore, to be that of separating and conveying out of the system the nitrogenized matters derived from worn-out tissue, withdrawing them from the blood during its circulation, and excreting them in solution in the urine.

A considerable proportion of the urinary constituents are soluble in water or an alkali, while others require that some acid should be present to effect their solution. The most important of the former are urea and the salts of uric acid: the latter class comprehends the phosphates of lime, magnesia, triple phosphate, &c.

The fact that the solid constituents of the urine are held in solution by agents differing widely in their chemical character, is a subject of considerable importance in respect to the formation of gravelly and calculous deposits. The salts of uric acid, for example, are more or less soluble in water and in the alkalies; but the addition of an acid, even weak acetic acid, to any of the urates in solution, causes their decomposition, with the consequent deposition of the uric acid, which is itself nearly insoluble in water and weak acids. This proves that an abnormally acid state of the urine, even if the free acid be of the weakest kind, is incompatible with the existence of the combinations of uric acid; hence the use of acid wines, fruits, or any matter which, by fermentation in the stomach, may be converted into an acid, as sugar, &c., may cause the precipitation of the uric acid either in the kidney or bladder; and according to circumstances, this deposit may take the form of gravel, or be concreted into a calculus.

Deposits of uric acid may, however, take place in the urine when no abnormal quantity of any other acid is present; and the deposition of the uric acid may be either pure or consisting of a combination of uric acid with an alkaline base, and would probably, in that case, depend upon its being generated in excess, or upon some diminution of the water of the urine. In persons who lead very sedentary lives, and take but little exercise, these uric acid deposits are frequent, owing, probably, to some interruption to the functions of the digestive organs, and also to the sluggish action of the skin, which, under those circumstances, is incapable of removing the nitrogenized matter ordinarily carried off in the perspiration. An excess of nitrogen is consequently conveyed to the kidneys, and carried by those organs into the urine, under the form of urate of ammonia, in quantity greater than can be held in solution; so that when the temperature of the urine is reduced below that of the body, the excess over the normal proportion is immediately depo-

sited. Excessive perspiration may produce a similar effect, although by a different kind of action. When a large amount of fluid is given off by the skin the bulk of the urine is diminished; but as nearly the average quantity of nitrogenized matter is conveyed to the kidneys, the urine secreted contains a disproportionate amount of uric acid, and then the deposit is formed on the cooling of the urine, exactly as under the circumstances before mentioned.

The earthy constituents of the urine, as I have remarked before, are insoluble in water; and some feeble acid, or acid salt, must be the means of holding them in solution in the urine. We have already seen that any free acid decomposes the combinations of uric acid (in which state only that substance is soluble in the water of the urine), precipitating it in the solid form. If, therefore, an acid be the solvent for the earthy phosphates in normal urine, it is evident it must be one of the feeblest kind; one, indeed, which cannot decompose the salts of uric acid. It has been supposed that chloride of ammonium is the solvent of the earthy constituents; but the very small quantity of this salt contained in the urine renders such an opinion scarcely tenable. The lime has also been thought to be present in the state of the super or acid phosphate, a very soluble compound, capable of acting as a solvent to other earthy phosphates, and believed by some to be the cause of the acid reaction of normal urine. Dr. Brett, however, some years ago, gave what appears to me to be a much more simple and rational view of the probable nature of the solvent of the earthy salts of the urine; he was, I believe, the first to shew that urine, when first voided, contained a considerable quantity of free carbonic acid, and this he regarded as the medium by which the phosphates of lime and magnesia were held in solution. That carbonic acid possesses the power of dissolving phosphate of lime or magnesia is readily proved, by passing a current of the gas through water in which those phosphates have been previously precipitated; after some time the whole of the phosphatic precipitate will be dissolved by the carbonic acid, and be held in solution until the temperature of the fluid is sufficiently raised to drive off the carbonic acid, when the dissolved phosphates are again precipitated.

If fresh urine be boiled in a flask furnished with a bent tube leading into lime-water, the latter will become turbid after a few minutes, owing to the action of the carbonic acid evolved from the urine; if the boiling be continued, a copious precipitate of carbonate of lime will be produced, and, at the same time, the urine will also become turbid, from the phosphates being precipitated, in consequence of the loss of their acid solvent. It has been suggested that the precipitate in the lime-water may be formed by means of carbonate of ammonia expelled from the urine by the heat; but this cannot be the case; for if a slip of litmus paper be placed in the lime-water during the passage of the carbonic acid and steam from the boiling urine, it will, after some time, be tinged red, indicating the presence of a free volatile acid, which in this case, so far as our knowledge goes, can only be carbonic acid.

The supposition that the solution of the earthy phosphates is due to carbonic acid explains the phenomenon of the precipitation of those compounds from the urine by boiling only. This is not uncommon in certain conditions of the urine; and it can easily be understood, that when the amount of phosphates exceeds the normal standard, the expulsion of even a very small proportion of the volatile acid solvent would cause their immediate precipitation.

As the phosphates are held in solution in the urine by the solvent power of an acid (in which it must be remarked they are dissolved only, without any chemical decomposition), it follows that the addition of an alkali, by which the acid solvent can be neutralized, will directly cause the precipitation of the earthy matter. Thus the addition of a small quantity of liquor ammoniæ to healthy urine will lead to the deposition of a cloud of the earthy salts. This fact is worthy of particular attention; for it shews us that the two classes of urinary constituents, viz. the salts of uric acid and the earthy phosphates, are held in solution together only so long as a certain equilibrium is preserved in the state of the urine, and that their properties are, as it were, exactly antagonistic to each other; the conditions most favourable to the solution of the one class being entirely incompatible with that of the other. For instance, the alkalies constitute the most effective means for the solution of uric acid and its salts, while at the same time the presence of a free alkali in the urine causes the earthy phosphates to be thrown down from solution. On the other hand, if the urine be rendered preternaturally acid, we have exactly the opposite effect produced; the acid favours the solution of the phosphates, which cannot be precipitated in its presence; but it decomposes the salts of uric acid, which latter substance is almost insoluble in water or the weaker acids, and therefore a deposit of red gravel or the formation of a uric acid calculus, is the result.

These, as I have said before, are points worthy the closest attention, because they evidently materially affect the curative indications in gravel or stone. For example, to administer acids where there is a tendency to uric acid deposit would, of course, be to increase the evil; and in like manner alkalies, which in such a case would be indicated, would, when there was a tendency to the deposition of the phosphates, promote that tendency, and be productive of great ulterior mischief. In the administration of remedies, when there is a liability to urinary deposits, it must be remembered that the salts of the vegetable acids with alkaline bases operate as alkalies, as they undergo decomposition in the stomach, and are converted into carbonates.

Now it is clear, that although in a perfectly healthy state, the balance between the solid constituents of the urine and their solvents is so adjusted, that the latter are always in sufficient excess to maintain the solid matter in a state of solution; still it is to be remembered that this balance is easily disturbed; so easily, indeed, that the slightest interruption to the functions of the skin or digestive organs is sufficient to produce certain abnormal changes in the

urine, leading to the precipitation of one or other class of its constituents. If we examine into the physical character of the formation of calculi, we shall find that the different constituents are never deposited simultaneously; that is to say, we never find a mixture of uric acid or urate of ammonia with earthy matter deposited in one general mass; but when these various substances are found in the same calculus, they are deposited in distinct laminæ, separable from each other. This fact serves to prove that the deposits of the urates, uric acid, or the earthy salts of the urine, are formed under circumstances dissimilar to each other, and under conditions supervening, and never co-existent. It is not difficult to understand how these different conditions of the urine may so completely replace each other, as the structure of almost all calculi affords evidence of their constantly doing. It seems to me that calculous concretions are most liable to occur when the urine is in a state of close equilibrium, or possessing, at the utmost, only just sufficient solvent power to maintain its constituents in solution at the temperature of the body. The condition I speak of may arise, as we have already seen, from a diminution of the fluid part of the urine, from increase in the secretion of uric acid, or from a deficiency of the solvent of the earthy phosphates. Under any of these circumstances, the precipitation of certain of the urinary constituents will be induced by very slight causes; such, for instance, as the presence of a foreign substance in the bladder capable of acting as a nucleus.

As the slightest causes are sufficient to produce that peculiar condition in the urine, in which it is prone to deposit uric acid or urate of ammonia, it will be found that calculi of those substances are much more frequent than any other kind. It is, however, said, that calculi composed wholly of urate of ammonia are rare in individuals who have passed the age of puberty; but an examination of the collections of calculi contained in various museums shews that about two-thirds of the whole number consist of stones, the chief or fundamental part of which is uric acid.

When a foreign body, accidentally introduced into the bladder, becomes the nucleus of a calculus, the deposit formed upon it almost invariably consists of the earthy constituents of the urine; this has generally been accounted for upon the belief that the foreign substance causes great irritation of the lining membrane of the bladder; and it is stated that, under these circumstances, phosphate of lime is secreted in increased quantity, and poured into the bladder mixed with the vesical mucus. The tendency to the deposition of the phosphates upon a foreign nucleus may however depend, at least in some measure, I think, upon a different action. It is well known that any solid body, having an uneven surface, and offering points and asperities, will cause the rapid evolution of carbonic acid from any fluid impregnated with that gas: thus, for example, if into soda water, in which the effervescence from the spontaneous liberation of the gas has completely ceased, we throw a few pieces of cuttings of metal, or even bread or wood, gas will again begin to

be given off from the fluid. If, then, we consider (and I think it scarcely admits of doubt) that carbonic acid is, at least in some measure, the solvent for the earthy constituents of the urine, it will at once be seen that the presence of a foreign body of irregular figure may cause the gradual but continuous discharge of the carbonic acid from the urine; and as the liberation of this gas takes place from the points presented by the foreign substance, it follows that the earthy matter thus deprived of its solvent is precipitated upon that body. This precipitation takes place, however, with extreme slowness; so much so, that the particles aggregate as they are deposited upon the nucleus. In addition to this natural aggregation of particles (which may always be observed when solid matter is deposited from a solution by slow degrees, even when regular crystals are not formed) the vesical mucus seems to operate as a kind of cement, assisting in the consolidation of the calculus during its formation.

Although, in almost every case in which a hard foreign substance forms the nucleus of a calculus, the deposit consists of the earthy phosphates, in some instances a foreign substance may lead to the deposition of uric acid or urate of ammonia. The nature of the matter thrown down from the urine under any circumstances must, however, depend upon the state of the constitution and upon the condition of the urine itself, so far as regards the relation of the solid matters to their different solvents. If the foreign substance present in the bladder be of irregular figure, and of a roughened surface, so that it is likely to produce much irritation in the mucous lining of the bladder, or, according to the view I have stated, to cause the slow evolution of the gaseous solvent of the earthy matter, then the deposit may consist of the phosphate of lime or magnesia, or the oxalate, or even carbonate of lime, as the case may be. If, on the other hand, the foreign substance be not of a nature to produce great irritation of the bladder, or to induce the liberation of gas; and if at the same time the fluid portion of the urine be from any cause diminished, or uric acid generated in excess; the deposit would probably consist of uric acid or the alkaline urates. Of course, as I have already said, the nature of the deposit must be materially influenced by the state of the constitution; and where calculi form spontaneously, that is to say, without any apparent nucleus being present, the kind of deposit will entirely depend upon constitutional causes, but under the former circumstances, that is, when stone is originally induced by the presence of a nucleus, the kind of stone, at least so far as its internal position is concerned, seems to depend in great measure upon the nature of that nucleus.

Calculi in the kidneys frequently follow mechanical injury to those organs, and no doubt often owe their origin to the presence of small portions of coagula, which, acting as nuclei, determine the deposition of the solid matter of the urine. When this is the case, it will generally be found that such calculi consist, at least internally, of uric acid or urate of ammonia; for when renal calculi consist of oxalate of lime, the circumstances of the case generally go to prove that they are entirely produced by constitutional causes.

When uric acid or urate of ammonia form the central part, or indeed the general mass, of a calculus, it very often happens that the external layers consist of phosphate of lime, of the fusible compound of phosphate of lime and ammoniaco-magnesian phosphate, or of the latter substance exclusively; but calculi consisting entirely of earthy matter are comparatively rare.

When the phosphate of lime is spontaneously deposited in the bladder, there appears to be, generally speaking, considerable impediment to its concreting into a solid mass. I have met with many cases in which the urine has been charged with an immense quantity of thick ropy mucus, which, upon chemical examination, was found to contain a great deal of earthy matter. Whether, in these cases, the effusion of the mucus was caused by the irritation arising from the presence of the phosphate of lime, or whether the deposition of the latter substance was produced, as is generally believed, by the reaction of the purulent mucus upon the urine itself, is a matter for question. I am inclined to entertain the former view, as in none of the cases I have mentioned was the urine alkaline, nor did it present any indications of putrescence. One thing is, however, certain, that the presence of the mucus affords an effective obstacle to the aggregation of the earthy particles, which remain completely suspended in the thick fluid, and pass off readily with the urine during micturition. I have several times collected the mucus from such urine, and found it to contain, after being dried, from twenty to thirty per cent. of bone earth, and, in one instance, as much as thirty-four per cent.

The phenomenon of the deposition of the phosphates from the urine appears to me to be indicative of a change in the constitution very analogous to that which is generally observed to be concomitant with advanced age. This change is doubtless, in both cases, the result of diminished vitality; and we therefore perceive, that causes operating as a means of lowering the vital power, would at any period of life, produce a condition similar to that which, in age, occurs spontaneously as the result of the natural exhaustion of the eliminating powers.

When the system is in that state of vigorous health arising from the due performance of the various animal functions, a certain balance is preserved between the deposition of the matters constituting the different parts of the body; so that, for instance, the formation of bone and muscle proceeds in a regular ratio, neither being in excess; and if the food contains more of any particular constituent than is requisite to preserve the normal equilibrium, that excess is rejected un-assimilated and is conveyed out of the body as excrementitious matter. As age advances the muscular system becomes less and less active, it diminishes also in bulk, and then the equilibrium between the bones and muscles is destroyed; the tendency on the part of nature is still, however, to maintain the natural equilibrium; consequently there seems to be established an effort to reduce the size of the bones by the absorption of the bone earth; and hence it is, that, in the bones of the aged, the proportion of phosphate of lime is always much less than in the bones of middle

life. In old people, as the activity of the muscular system decreases, so also does the circulation become more or less enfeebled; so that, even if there existed no tendency to reduce the quantity of phosphate of lime in the bones by absorption, it would still be impossible for them to receive the normal quantity of that compound; for the blood, which is the carrier of all the constituents of bone, as well as of the other tissues, has lost the vigorous impulse that was wont to propel it through the capillaries. It must, however, be remembered, that the blood itself still continues to receive a supply of the phosphates from the food; and as they cannot be conveyed by the capillaries to increase the osseous system, they become deposited in other tissues; and we accordingly find in old age earthy concretions forming in the arteries and other solid structures, but most frequently in the urine. Now it is evident to me, that this condition arises entirely in consequence of diminished vital energy; and I believe that whenever a similar disposition to phosphatic deposit occurs, either in youth or middle life, it arises from a precisely similar constitutional state, induced by the operation of any cause capable of depressing or exhausting the vital powers. Excessive fatigue, long-continued mental anxiety or labour, disturbance to the function of digestion, or chronic disease, may, any of them, be capable of producing this phosphatic diathesis. In examining into the statistics of urinary calculi, I think I have found that the phosphates seldom constitute the primary deposit until after the middle period of life—until, indeed, the action of the arterial system has become somewhat sluggish; and when the phosphates form calculi at an earlier period, I have no doubt that investigation would prove the individuals, in all such cases, to have laboured under great constitutional debility. When the phosphates are deposited upon a nucleus of foreign matter accidentally introduced into the bladder,—as, for instance, a piece of bougie,—I believe the action to be dependent upon the principle I have already stated, viz. the mechanical effect of the extraneous matter upon the urine; for in such cases the constitutional disturbance is seldom commensurate with the amount of phosphatic deposit, and moreover, the tendency to the deposition of the phosphates appears to cease immediately upon the removal of the foreign substance which originally acted as the nucleus.

In early life there seems to exist a condition of the system exactly the converse to that just described as peculiar to old age. In the former case the vital energies are at their maximum activity; the assimilation of the constituents of muscle and the soft parts of the body seem then to be carried on with greater rapidity than that of bone; consequently there is an excess of those tissues rich in nitrogen. Hence in youth there exists a tendency to the formation of uric acid in an abnormal quantity; and this substance, either alone or in combination with ammonia, will, I believe, be found to constitute by far the greater proportion of urinary calculi, not only in youth, but even at later periods, so long as the system remains in a vigorous state; and as these uric acid deposits

are frequently formed without any apparent interference with the normal performance of the animal functions, they seem scarcely to constitute a diseased condition.

Among the constituents of urinary calculi, those that I have already spoken of,—namely uric acid and its compounds, and the earthy phosphates,—may be derived at once from the urine, without that fluid having undergone any chemical change; it being, I believe, only necessary that these substances should be increased in quantity as a result of their inordinately free elimination from the blood; a circumstance which experience teaches us is by no means unfrequent under peculiar conditions of the system. These compounds must, therefore, be regarded as composed of the normal constituents of urine. There is, however, another class of substances sometimes found in the urine, which are altogether different from those just now mentioned, and which do not enter into the composition of that fluid in its normal state, and are, indeed, never found in perfectly healthy urine, but are the product of some vitiated vital action which causes the mal-assimilation of the ingesta. Of these compounds, oxalate of lime is no doubt that most frequently met with, both in the urine itself and under the form of calculi. Indeed, it has been shewn by Dr. Golding Bird that crystals of oxalate of lime are commonly met with in the urine of patients labouring under chronic dyspepsia; and it is well known that the mulberry, or oxalate of lime calculus is one of comparatively common occurrence.

From the observations of medical authorities on the circumstances attendant upon the formation of deposits of oxalate of lime, and of the earthy phosphates, it would at first sight appear, that, in both cases, the cause of the abnormal state of the urine may be traced to extreme constitutional depression and loss of vital energy. There is, however, in my opinion, a strong distinction to be drawn between the condition of the system which leads to the deposit of the earthy phosphates, and that in which oxalate of lime is generated. The latter substance is a product of the vitiated action of organs naturally employed in the elaboration of the ingesta into those matters required for the formation of the various parts of the organism, and the supply of the waste of tissue always going forward during life. In consequence, then, of some interruption to those vital chemical changes, by means of which the refuse nitrogenized matter of different organs is brought into that condition in which it can be excreted in solution in the urine, oxalic acid is generated. This substance must be looked upon, therefore, as one foreign to the animal system in its normal state; and its formation is doubtless dependent upon, and symptomatic of, that peculiar constitutional depression which arises from a defect in the function of the digestive organs, or perhaps in those of respiration, if the supposition of some chemists be correct, viz. that the generation of oxalic acid depends upon the imperfect oxidation of those nitrogenized matters which, in the normal state, are eliminated by the kidneys, under the form of uric acid and urea.

The formation of oxalic acid is traceable, then, to a modification of those chemical changes, to which we owe, under all circumstances, the elimination of the natural constituents of the urine from the blood; and we can scarcely fail to perceive why (although the circumstance had so long escaped the notice of medical practitioners) the formation and presence of oxalate of lime should be very frequent in the urine of individuals labouring under any abnormal condition of the digestive, or of the respiratory organs.

I have lately examined the urine of a boy labouring under phthisis. It afforded a strong support to the above view, for it contained an immense number of crystals of oxalate of lime, and also the cystiform appearance mentioned by Dr. Prout as indicative of serious functional disturbance, or organic lesion. The appearance I allude to resembles strings of egg-like bodies united by their extremities, three, four, five, or six together.

Any circumstance which produces interruption to the process of assimilation, and secondarily, perhaps, to that of respiration, favours the formation of oxalic acid; and although, according to that view, which refers the generation of this substance to the imperfect oxidation of uric acid, food can produce no direct effect upon this phenomenon; still, in many cases there can be no doubt that the character of the food does produce an immediate influence, and that oxalate of lime taken into the stomach does, like some other saline matters, find its way at once into the urine. And it seems to be probable, when we reflect upon the close connection between certain of the chemical principles of the food and oxalic acid, and upon the facility with which such substances as starch, gum, and sugar are artificially convertible into oxalic acid, that an analogous change may take place under the influence of vital chemical action, and oxalic acid be generated as a direct product from the food.

In cases where the deposition of phosphate of lime occurs, the circumstances will, when closely examined, be found very dissimilar from those under which oxalate of lime is produced. It is true that, both where phosphate of lime and oxalate of lime are deposited, there exists in a high degree loss of vital power; but this condition appears to arise from very different causes. When the earthy phosphates are precipitated from the urine, the principal concomitant symptom is extreme nervous depression; and therefore any cause operating upon the nervous system is generally sufficient to produce the deposition of phosphates from the urine. I have already stated my belief, that, in almost all cases, the phosphates are deposited from the urine itself, and not eliminated by the mucous membrane of the bladder; and it is easy to perceive, that where the nervous influence has become much diminished, that peculiar property possessed by all organic fluids (when existing in the organism in which they are secreted) to resist ordinary chemical change is very materially lessened. Hence the fluid is prone to rapid decomposition; and such appears to be the case with the urine. No matter what the cause may be, whether accidental or idiopathic, where the nervous energy becomes much weakened, the conservative

power, if I may employ the expression, is proportionally lowered; and, under these circumstances, the urine passes rapidly into a state of change, its elements re-arranging themselves, to produce an alkaline condition, which, of course, leads immediately to the deposition of the earthy phosphates, under the form of the triple phosphate, phosphate of lime, or a mixture of both. Hence it is that lesion of the spinal marrow always produces alkaline urine, by destroying the nervous energy of the coats of the bladder, as well as that of the urinary organs generally.

Where the nervous influence is much depressed, I have often remarked that the urine itself is liable to undergo decomposition very soon after evacuation; and indeed I almost question whether, in some cases, although the urine did not manifest any indications of putrescence, incipient decomposition had not commenced in the bladder; for I have known such urine to be filled with infusoria (vibriones) within a very few hours after it was passed. I have particularly noticed this in cases of paralysis, and in those of great depression accompanying long-standing disease of the prostate gland in old people. In phosphate of lime we have one of the normal constituents of the urine, which, without undergoing any chemical change itself, becomes altered in its relations to that fluid, in consequence of induced changes in the chemical nature of certain of the organic urinary constituents. In oxalic acid, on the other hand, we have a substance abnormal to the system, produced by a defect either of the primary assimilation of the food, or in those chemical operations by which the worn-out tissues are brought into that state in which they can be conveyed out of the system, through the agency of the kidneys.

In the generation of oxalic acid we perceive the manifestation of a vital action, although probably one of diminished intensity, in comparison to that natural to the organism. In the deposition of earthy phosphates we have the ultimate result of remote changes acting on the constitution generally, and reducing the vital power of every portion of the living body, so that it is no longer capable of withstanding the influence of the usual chemical changes to which all dead matter is subject. The deposit of the earthy matter is the effect of the dissolution of those forces by which the normal condition of the urine is maintained, and is the result of a change, in which they play no part themselves, their physical condition alone being modified according to the nature of the influences by which they are surrounded.

The formation of oxalic acid is only one out of many instances of the generation of peculiar abnormal compounds, in consequence of some interference with the ordinary vital chemical changes that constitute the medium of the removal of the worn-out tissues of the body.

The formation of sugar in the urine, in diabetes mellitus, of the substance termed xanthine or uric oxide, and of the remarkable compound, cystine, are all examples of vitiated vital chemical action.

Cystine affords perhaps the most striking instance of this kind, it being extraordinary on account of the large proportion of sulphur it contains—above one-fourth of its whole weight. Although the nature of the chemical changes by which cystine is formed is not at all understood, it is, I think, evident that this substance must be derived from the metamorphosis of albuminous matter, and is probably a means adopted by nature to remove an excess of sulphur, which ought normally to have passed away by some other channel. It appears from the researches of Dr. Golding Bird and others, that the formation of cystine is generally the result of a peculiar constitutional deterioration, which is often hereditary, or at least common at one time to several members of the same family.

I believe that the formation of urinary concretions, generally speaking, depends less upon defect in the urinary organs themselves, than upon an abnormal state of the constitution; and the processes of digestion, respiration, and exhalation from the skin bear so important a relation to calculous disorders, that the constitutional treatment certainly deserves as much attention from the surgeon as the study of the minutiæ connected with the mere operation, which seems, however, to be too generally considered as the only part of the subject with which he ought legitimately to deal.

As acids are known to produce the deposition of uric acid from its solution in the urine, it is generally believed that the effect of acid taken into the stomach is to increase the tendency to the formation of uric acid deposits in constitutions in which that diathesis prevails; but it is a matter of question whether the acids taken into the stomach are capable of passing into the urine in a free state. I think that both acids and alkalies undergo chemical change, and only produce their effects through their influence on the constitution. There can be no doubt that the carbonates of soda and potash, and liquor potassæ, are useful in a uric acid diathesis; but whether they merely act by neutralizing the acid of the urine, or by altering the peculiar condition of the blood, I am not prepared to say: this I know, that blue pill, colchicum, and opium produce the same effect as alkalies, even when there is no evidence of direct gouty tendency. These remedies seem to act upon the assimilative organs, leading to the formation of more healthy blood, and so influencing the condition of the urine.

In the uric acid diathesis, low diet and vegetable food are advisable, and some vegetable acids seem useful, although all substances liable to generate acid in the stomach must be scrupulously avoided. A proof that the use of vegetable acids does not produce any tendency to stone is, I think, proved by the fact, that calculous complaints are almost unknown in the cider districts. The use of the warm-bath is also highly beneficial in the uric acid diathesis, as it promotes the functions of the skin; and the sympathy between the skin and urinary organs is clearly demonstrated from the frequent co-existence of cutaneous eruptions and disease of the kidney. It is seldom that the sediment in the urine consists of pure uric acid; and this is likewise the case with respect to calculi, in which

the uric acid is scarcely ever pure. When alkalies are administered, they should not be taken, as Sir Benjamin Brodie has well remarked, in less than three or four hours after a meal, or they are liable to interfere with the process of digestion, by neutralizing the free acid of the stomach. The quantity of alkali given must also depend upon the peculiarity of the patient's constitution. Should the treatment not prove effectual in preventing the formation of a stone in the kidney, it generally happens that the concreted matter soon passes into the ureter, and along that channel into the bladder; and if the stone be sufficiently small, it may ultimately make its escape through the urethra.

Before the invention of lithotrity, the only means employed for the removal of small calculi consisted in dilatation of the urethra by large bougies, or in their extraction through the urethra by the instrument invented by Sir Astley Cooper, and known as "Sir Astley Cooper's forceps:" by these forceps great numbers of small calculi were, in many cases extracted. But I believe that there is less danger in the use of the lithotrite than in either of the above plans, as the excessive stretching of the urethra, and the frequent introduction of the bougie or forceps seems more likely to set up irritation of the bladder, than the skilful use of the crushing instrument. In three instances I have crushed a stone, and perfectly cured the patient, by one operation with the lithotrite.

The choice between lithotrity and lithotomy is often one requiring considerable judgment on the part of the surgeon; and this is a question in which a knowledge of the chemical nature of the stone must always be of great service: not that the chemical composition of the stone has anything to do with either of these operations directly; but the physical character of the different kinds of stones varies as much as their composition, and that character can be judged of pretty correctly as soon as the composition of the calculus is known. Uric acid, for instance, often occurs in a form in which it easily breaks into pieces under a crushing force; and the crystallized variety of the triple phosphate also crumbles away completely under pressure. By means of the lithotrite it is easy to comminute such stones as these; but there are others which yield with great difficulty to the action of that instrument. The oxalate of lime calculus is of this nature: the tuberculated portions break off readily enough, but the mass of the calculus is crushed with great difficulty; and when it is broken, the fragments are so sharp that they are likely to produce extreme irritation in the bladder, and then a secondary action may be established, in which these fragments become the nuclei of other calculi, or at least cause the deposition of fresh quantities of earthy matter from the urine. Whatever may be the nature of the stone, I think it is always inadvisable to employ the crushing process if there be evidence of a tendency to the deposition of the earthy phosphates. Of course this must be judged of by the urine; and unless the tendency can be removed, it seems to me that the operation of lithotomy should be preferred; for I believe that the incision through the prostate

gland causes less irritation than the use of the lithotrite, and the consequent presence of the calculous fragments. The advantage of lithotomy is, that the chief source of irritation is removed at once; and although there may remain the constitutional tendency to deposition from the urine, there would be no nuclei to promote that tendency, and it would be therefore the more likely to be counteracted by therapeutical agents.

Whenever, also, from the diathesis of the patient, the state of the urine, and the examination by the sound, the stone is believed to be of that kind which is crushed with difficulty, the operation of lithotomy is certainly indicated.

With regard to the constitutional condition of the patient, the same considerations would weigh with the surgeon in lithotomy as in lithotrity; and one of the chief points would be, to overcome, before the operation was performed, the constitutional tendency, if any still continued to exist, to further deposition. Generally speaking, the physical character of the stone bears little immediate relation to the operation of lithotomy; but I may say that the kind of stone most suitable to lithotrity is perhaps that most difficult to deal with in lithotomy. The more friable a stone, and the less the cohesion of its particles, the better suited it is to the lithotrite; but such a stone would be more likely to increase the difficulty of the operation in lithotomy, as it would probably crumble and break away when seized by the forceps, and so lead to the necessity of some modification of the operation, in order to get rid of the whole of the fragments. The hardness of a stone can be no objection in lithotomy, unless it be too large to be removed whole, in which case great irritation may be produced in the efforts of the surgeon to break it while in the bladder. I believe that the operations of lithotrity and lithotomy may, in a certain manner, be very advantageously combined. For instance, it sometimes happens that a stone is so large, that, although it may be removed without previous crushing, it requires a very extensive incision through the prostate. In such a case, I think that a strong kind of lithotrite may be introduced through the wound, and the stone easily broken down, without producing so much irritation as would arise from the extension of the wound by the forcible extraction of a large stone.

Guy's Hospital Reports, Vol. VI., October 1849, *p* 2.

85.—*Case of Stricture attended by Profuse Discharge.* — By H. Smith, Esq., London.—[In this case there was such profuse discharge, that gonorrhœa was suspected. The patient had previously had stricture, and as he completely negatived the idea that it was gonorrhœa, Mr. Smith suspected that he might have stricture remaining. He says:]

I requested permission to examine the urethra, and, on doing so, I discovered two irritable strictures. I told him that this was the cause of the discharge persisting, and that it was necessary for him

to come to London every week, to have the bougie passed. I, at the same time, desired him to refrain as much as possible from sexual connexion, and from all other species of excitement.

I need not enumerate the particulars of this case, which was troublesome to treat,—for the urethra was so excessively irritable, that about three weeks after this, I could only introduce, with some trouble, a No. 4 bougie. In a short time, however, a good-sized instrument could be introduced, the discharge diminished, and, at the end of two months, I passed No. 12 bougie; all discharge had ceased. He had latterly used a weak solution of acetate of zinc, in order to diminish the irritability of the urethra, and hasten the cure.

I advised him to return to me in another month, in order that I might ascertain if the cure was satisfactory. He visited me last week. The urethra admitted No. 12 bougie, and the discharge had never returned.

The points of interest in this case are obvious; it particularly indicates how careful the surgeon should be in giving a decided opinion respecting the nature of a discharge, when the patient is a married man, and denies having placed himself in a position to get infection. It is true, that the surgeon is frequently wilfully imposed upon by his patient; at the same time an educated and sensible man, if he have any confidence in his medical attendant, will generally tell the truth.

I am by no means surprised that the discharge in this instance was taken for and treated as gonorrhœa, for the similarity was so great that it was almost impossible to give any other opinion; for, although it frequently happens that a thin gleety discharge is produced by stricture, one of a purulent or muco-purulent character, exactly like that of a common clap, is rare as an effect of this altered condition of the urethral canal. This case strikingly shows the necessity of examining the urethra with an instrument, if a discharge persists for a length of time, and resists ordinary remedies; for in many such cases the discharge is kept up by the stricture, and unless this be overcome, no permanent cure can be expected to result from remedies by the mouth or by injections; but as soon as the obstruction is fully overcome, the running, whether it comes from the urethra or neck of the bladder, will readily give way. As an instance of the dependence of the discharge upon a stricture, and its rapid departure as soon as this has been treated, M. Leroy d'Etiolles mentions, in his work on stricture, a case of a gentleman suffering under two bad strictures. Here there was such a profuse discharge of thick, mucus-like, false membrane, that the unfortunate patient almost lived upon a perforated chair. This lasted for a year, when the stricture was discovered, and treated properly; and in eight days the discharge ceased, and since ten years has not returned.

Medical Times, Aug. 4, 1849, *p.* 97.

86.—*Account of an "Anti-Stricture Injector."*—By T. Machell, Esq.—[Mr. Machell had a patient with a stricture so irritable that he could not bear the introduction of a catheter or bougie.]

Having previously, (he says,) found much benefit, in some cases of spasmodic stricture, from passing a stream of water into the bladder, which not only relieved the contraction of the urethra and the adjacent parts, but diluted in an eminent degree both the calculous matter and the acid contained therein, as well as in the connecting passages, a proper apparatus for the purpose was procured, giving an impulse to the fluid suitable to the degree of resistance which the obstructions in the urethra occasioned. A small ivory pipe was employed, of sufficient dimensions to fill up the orifice of the passage into the bladder, which was at the time exceedingly irritable and inflamed, and of just sufficient length to reach the stricture. The strength of the stream of water to be directed into the urethra, and against the strictured part, was regulated to that of an ounce, but could be used much more powerfully, if required. This slight stream could not, at the first attempt, be endured, having caused a degree of shivering, and a partial paralysis of the right thigh; yet, notwithstanding the unfavourable sensation, I was confident that a portion of the water had gone through the stricture into the bladder, although it seemed to regurgitate into the basin beneath. I desired him to attempt to pass urine, which he was very reluctant to do, from the pain he had on previous occasions experienced from passing, as before described, only a few drops at a time of thick bloody mucus, of an irritating quality: yet he at length consented, and was agreeably surprised on hearing the urine, or rather water, pass off in a comparatively powerful stream into the utensil, nearly clear, accompanied with but little pain, and in quantity about two ounces. This occurred at some distance from town, and as I was obliged to return that night, I gave him some aperient medicine, and revisited him on the following day.

I found that in the interval he had voided urine in much less pain, and in a larger stream. The success thus obtained increased his confidence, and enabled me to repeat the operation. The bladder was again injected with water at a tepid degree, but with only about half the impulse. By means of the apparatus, about half a pint of water was introduced; at first, the stream occasioned considerable pain, but, by applying the stop upon the pipe, and suspending the force of the stream, the pain subsided, and I was again enabled to turn on the fluid; which was accomplished without any inconvenience, and the whole operation was completed without the necessity of withdrawing the ivory tube even for an instant.

In the space of two months, all the distressing symptoms subsided, and although, from unforeseen causes, there was evidently a disposition of the urethra to contract, yet the same simple means were had recourse to, keeping in mind the regulation of the impulse of the stream, agreeably to the resistance of the spasm, and other

obstructions in the canal. The acrimonious urine, and the accumulated mucus, on being diluted, passed off freely without the straining which he had experienced for a great length of time.

During the space of two years my patient has remained quite comfortable, without requiring any further use of the instrument; and should any return of strictures take place, he will then have recourse to the same process, which was attended with very little inconvenience to himself, and with a success which he never anticipated.

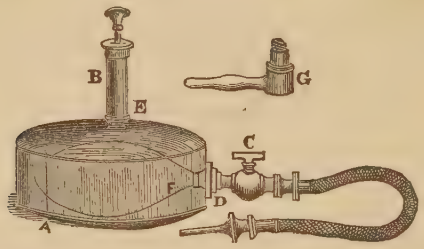
It will thus be seen that where the bougie or catheter completely failed, an easy and effectual cure may be attained by the simple application of water administered by means of the "Anti-stricture Injector."

Lancet, Aug. 4, 1849, *p.* 134.

87.—*On the Treatment of Stricture.*—By T. Machell, Esq., London.—[Mr. Machell makes the following observations on the treatment of strictures:]

In the use of distending the urethra by means of bougies, or other mechanical contrivances, however gentle in their application, they cannot fail to cause a degree of stimulus, which, on some urgent occasions, requires to be discontinued until the disturbed state of the implicated organs has been quieted by medical treatment, when the mechanical process of dilatation may again be reverted to with increased advantage. That such a sympathy exists between the strictured parts, which are thereby rendered unfit to perform their office, and some of the viscera, has, by repeated experiments, been sufficiently proved; for in proportion as the membranous lining and the corpus spongiosum urethræ become contracted, these parts are considerably more condensed and thickened into the consistence of cartilage, whilst the coats of the bladder increase in the same ratio in strength, for the purpose, as it were, of giving additional impulses to the urine when impelled through the contracted canal. A stricture of the urethra is frequently in extent only as if contracted with a silk thread, but at times, though seldom, it is about an inch in length. A permanent stricture generally appears to encompass the canal with a small perforation in the centre, and at times, the stricture is on one side. The middle of a long stricture is sometimes larger in diameter than at the ends, and frequently the urethra assumes a tortuous direction; or a membrane, denominated a bar, will extend across the canal. All these remarks on the variety of strictures,—which require different modes of treatment,—by bougies, catheters, natron exsiccatum, argenti nitras, kali purum, and unguentum hydrargyri fortius, are introduced, in order to show the enormous amount of irritation which would be produced in an already excited state of the urinary organs, where extraneous deposits are of too stimulating a nature to require any addition of inflammatory excitement to remove the cause. With a view of mitigating the existing symptoms by the

removal of all unnatural secretions, and relaxing spasmodically contracted canals and passages, the soothing effects of heat and moisture are employed, as being well-known agents for the purpose. Nothing but an easy and perfectly controllable method of administering them is wanted, a desideratum which I have found attained by means of the instrument referred to, a diagram of which may in some measure serve to point out its construction and mode of application.



A, is a vessel made sufficiently strong, and of material impervious to air in a condensed state; B, a syringe for compressing atmospheric air into the said vessel; C, is a stopcock which screws into the opening at D, and, in conjunction with the valve E, serves to secure the injection, after being poured into the vessel through the opening, and to regulate the stream by diminishing or increasing the power from one drachm to the weight of several pounds; G, is one of the vessels for containing compressed air, or any of the gases. When the injector is to be used, unscrew the stopcock, C, and fill the vessel quite full of the liquid to be injected, then return the stopcock into the opening tolerably tight. The air-syringe, B, must now be screwed into E, which, being worked to the extent of twenty or thirty strokes, renders it ready for use. On introducing the ivory pipe in the usual way, the injection can be administered with any required degree of force, and in any position of the body, without the aid of a second person, and without the introduction of the slightest particle of atmospheric air.

Lancet, Aug. 25, 1849, *p.* 203.

88.—*On the Danger of using Gutta Percha Catheters.*—By Cæsar Hawkins, Esq., Surgeon to St. George's Hospital.—[Mr. Hawkins states that however carefully catheters of gutta percha may be used, yet they are liable to break, on account of the mode of their construction. He says,]

The catheter is made, it would appear, of a slip of gutta percha nearly an inch wide, and of the necessary length for making the catheter, which is twisted round a stilette, so as to make the edges

of the slip join together, in which position a moderate degree of heat makes them cohere to constitute a tube, when the stilette is withdrawn. Of course, if this slip is screwed up in one direction, the edges are pressed together, but when twisted in the contrary direction, unless made to cohere very firmly, the catheter is readily untwisted into the original long narrow slip of the material employed, and probably the warmth of the body facilitates this separation of the edges of the slip. A second catheter, which my patient used on only one occasion, was thus opened at one point of junction, and was easily unrolled for its whole length; others are not so easily broken up, but all must be dangerous.

[In illustration of the danger resulting from the employment of these instruments, Mr. Hawkins relates a case in which the patient broke the catheter in his bladder about four inches from the point, and the fragments were not extracted without considerable trouble.]

Medical Gazette, Oct. 26, 1849, *p.* 699.

89.—ON VASCULAR TUMOURS AT THE ORIFICE OF THE URETHRA.

By H. B. Norman, Esq., Surgeon to the Marylebone General Dispensary, &c.

[In addition to his own observations upon the subject of these tumours, Mr. Norman gives an able *resumé* of the opinions of various authors who have previously described them. He gives the following description by Sir Charles Clarke:]

"The tumour," he says, "is *seldom firm*; of a florid scarlet colour; if violence be offered to it, blood of the same colour is effused: it is somewhat *granulated*, exquisitely tender; appears on accurate examination to shoot from the inside of the urethra; seldom acquires a large size; its attachment is slight, and it is so moveable as to appear almost like a detached body lying upon the part. It gives rise to great suffering during micturition, and to a vaginal discharge. It is common to the married and the unmarried; most frequent in the young." The tumour was never seen by Sir Charles Clarke "in a person above the middle age." He notices that a similar kind of disease existed occasionally "in the interior of the canal, giving rise to the symptoms of tenderness on pressing the urethra, and a general swelling of the part;" and refers to such a case successfully treated by Sir James Earle by means of large bougies.

In most systematic works on diseases of the genito-urinary organs, and especially in those which treat of the female organs, these tumours are described; and in those published since Sir Charles Clarke's work, the name given by that author is adopted.

Thus Burns says:—"Excrescences may, notwithstanding the opinion of Morgagni, form in the course or about the orifice of the urethra, and generally produce great pain, especially in making water, on which account the disease has been sometimes mistaken

for a calculous affection. The agony is at times so great as to excite convulsions; and it is not uncommon for the patient to have an increase of her sufferings about the menstrual period. The tumour is vascular, florid, moveable, and exceedingly tender. When excrescences grow about the orifice of the urethra, they are readily discovered; but, when they grow high up, it is much more difficult to ascertain their existence." Dr. Baillie says "they cannot be known but by the sensation of a catheter passing over a soft body." Burns refers to many interesting cases, hereinafter to be cited. The late Dr. David Davis makes mention of these tumours, and gives a most interesting and complicated case in illustration. Sir B. Brodie limits their size to that "of a pea, or horsebean." He has met with many instances of them.

Dr. Ashwell thus treats the subject:—"This is by no means an unfrequent affection of the mucous membrane of the urethra. It usually attacks the young, whether married or single; is very rarely seen after the cessation of the menses; consists of a small *granular* tumour, generally pediculated and moveable, attached to one part of the margin of the meatus, or just within the urethra, *whose two prominent characteristics are great vascularity and exquisite sensibility*. Sometimes these growths are not moveable, but appear like several raised coalesced granulations, or occasionally two or more isolated independent ones may be seen near together. Every now and then they extend partially along the urethra, and may even be located at the neck of the bladder. They are covered by a delicate *pale* mucous membrane, which is broken on being touched during examination, and blood exudes from the soft, feeble, and injected capillaries. ..... If they interrupt or come in contact with the stream of urine, the act of passing water causes intense pain, and this is usually the first and most constant symptom of the complaint. After a time the urethra and bladder become irritable, and the frequent desire to micturate is suppressed by the dread of the pain which it excites. Sexual intercourse is intensely painful, and is therefore obliged to be suspended. Walking exercise is attended with suffering, and in fact any movement of the vulva, by slightly displacing or pressing upon the growth, produces instant pain. Leucorrhœa, and various sympathetic pains of the pelvis, loins, &c., are mentioned as accompanying the tumour.

Mr. Safford Lee has met with these tumours "in the young and old." The largest example he had seen (as large as a date) was in a woman above forty; and he had one under treatment much older.

He notices the occasional participation of the nymphæ in similar disease, its occasional extension along the urethra to the bladder, and also that the tumour is frequently divided at its extremity. Amongst the symptoms to which the tumour gives rise, he says, "One patient I saw had pains down the inner side of the thighs to the knees; and Professor Simpson told me of one case where pains n the soles of the feet were complained of."

Boivin and Dougés describe them as "fungoid tumours," occasioning great inconvenience in micturition—formed on the meatus, the borders of which orifice concur irregularly to their formation, rounded, though often granulous, varying in size from that of a pea to a cherry.

It will be seen that the urinary passage of the female is, in its whole extent from the external orifice to its termination in the bladder, liable to become the seat of excrescences, and that though the orifice is the most frequent seat of these growths, they are not very uncommon in the interior of the canal.

Though entitled to one common appellation, from their vascularity, they differ much in some other respects. Thus, in size, they may vary from a simple large granulation to a tumour as large as a date, or as a turkey's egg; in consistence, from something scarcely exceeding the solidity of jelly to a considerable degree of firmness; in external surface they may be perfectly smooth or granulated; in form flattened and sessile, rounded, and growing irregularly from the whole margin of the meatus, or pediculated and pendulous; sensitive to the highest degree, or as in one instance not even sensible. Their sensibility bears no relation to their size, unless it be in the inverse ratio; the smallest excrescence certainly gives equal pain and distress with the largest.

[One of these excrescences having been examined microscopically by Mr. Quekett, he gives the following account of it:]

A vertical section through the middle of the mass showed several papillæ of various sizes, which were very vascular, and surrounded by an investment of cuticle, which, with the papillæ, made up the entire mass of growth; at the part where the papillæ were situated, the growth was smaller than at the opposite extremity, as though it had been attached by a constricted neck or pedicle. The papillæ, no doubt, were largely supplied with nerves, as well as blood-vessels, but their presence could not be detected by the microscope. The growth may be said, then, to consist of hypertrophied papillæ, invested with a thick layer of cuticle, which projected from the general surface of the mucous membrane in a wart-like form. This connection with the papillæ will serve to explain the extreme sensibility generally possessed by these tumours.

[Mr. Norman concludes with some useful observations on the treatment of these tumours. He says,]

The most constant symptom of the disease is pain in the urethra, greatly augmented during micturition, which is sometimes suppressed by dread, and sometimes excessively frequent from the extension of the irritation to the bladder; augmented, also, by all movements of the vulva, by coition, and during the menstrual flux. A muco-purulent discharge from the vagina accompanies the growth, and there are occasionally severe sympathetic pains in the loins, pelvis, hips, and inside of thighs, and a discharge of blood leading to the idea that the case is one of calculus of the bladder. To prevent this mistake, which has been often made,

whenever the more common symptoms are present, and *not accounted for by a morbid condition of the urine*, careful examination of the vulva should be made before an opinion is given.

The external characteristics of these growths are sufficiently clear to prevent their being confounded with other swellings,—as verrucæ, prolapsus of the bladder, eversion of the mucous membrane of the urethra, or varicose tumour of this part, and still more with prolapsus uteri; a mistake which, in one instance, has nevertheless happened.

The treatment required is strictly local, and consists of such means as entirely destroy the growths and are most likely to prevent reproduction. These are cauterization, caustics, astringent lotions, excision, ligature, and pressure by bougies.

They are all applicable in different cases. If the growth is single or double, and not attached by a very broad base, it may be removed either by excision or ligature. The former has, perhaps, the most numerous advocates, and among them Sir B. Brodie and Dr. Ashwell, and was practised in several of the cases cited in this paper. To the latter I give a decided preference, along with Sir Charles Clarke, in consequence of the serious hemorrhage that often follows excision. In either case, a powerful caustic should be applied to the point from which the growth has been removed, to prevent its reproduction.

The actual cautery or caustics are only applicable primarily to very small excresences near the orifice, and to those which answer to the description of Dr. Ashwell,—*i. e.* having the appearance of several raised coalesced granulations, or two or more isolated ones; as a secondary means, they are applicable, as before mentioned, after excision or ligature. The more powerful the caustic, if proper care be used to prevent its spreading, the less suffering it produces and the more quickly it effects its purpose. Potass, nitric acid, and per-nitrate of mercury are the best. Nitrate of silver gives excessive pain, and is slow and inefficient.

Monthly Journal, June, 1849, *p.* 795.

90.—*On Varicocele.*—By M. Vidal de Cassis.—M. Vidal de Cassis has frequently occasion to treat affections of this kind at the Venereal Hospital. The process which he employs in the treatment is peculiar to him, and may deserve attention. He denominates it "*enroulement*," which we may translate by the term "torsion." A straight, lance headed needle is armed with a very flexible silver wire, of the diameter of a pin. The patient is in the upright position, and the surgeon commences by separating the varicose veins from the vas deferens, which he pushes backwards and inwards. The veins are grasped by the thumb and index finger with a fold of skin, and the needle is passed behind the vessels, being guided from the index to the thumb of the operator, and carrying the silver wire with it. The engorged vessels are now allowed to fall back, and the index and thumb of the operator pinch

up the skin in front of the enlarged mass of veins, and another silver wire, but of smaller diameter than the former one, is passed in the same manner. The patient is now placed in bed, the ends of the wires are cut off, leaving from fifteen to twenty centimetres, and the surgeon proceeds to twist the ends, which gradually brings the wires to bear on the chord; but as the torsion is continued, the veins are twisted round the metallic loop, and the testicle becomes elevated in proportion to the torsion.

Finally, a small plug is placed on the portion of skin between the two orifices made by the needle; the ends of the wires are twisted on this plug, and the torsion increased by means of a new instrument, which M. Vidal has just invented, and which Charriere has executed with his usual skill.

The advantages of this method, according to M. Vidal are, that it exposes the patient less to the chances of relapse, and is less painful than any other. The treatment usually lasts for three or four weeks.

Medical Times, Aug. 14, 1849, *p.* 93.

91.— *On Amputation of the Penis by means of the Actual Cautery.*—By M. Bonnet.—The actual cautery has been occasionally resorted to for the purpose of arresting dangerous hemorrhage after this operation; but during the last five years M. Bonnet, of Lyons, has employed it for the execution of the amputation itself, by which means he not only prevents hemorrhage, but what is of more consequence, phlebitis, and purulent absorption. He describes his success as very gratifying. After heating the iron, the patient is subjected to the influence of chloroform, and wet compresses are disposed around the penis. An assistant holds the organ by means of a forceps, and the surgeon, standing on the left of the patient, gently applies the iron perpendicularly to the axis of a healthy portion of the back of the penis, the division of the fibrous structure often requiring more than one such application. This accomplished, the iron is gently passed into the corp. cavernos., so as to coagulate the blood of its vessels, and the section is completed without loss of blood; four or five applications of the iron being wanted in all. When the cancer extends to the scrotum or abdomen, the operation is more tedious, as the ramifications must be followed by the cautery, though the pubis itself has to be reached. If the inguinal glands are affected, they should be removed by a bistoury, and the cautery applied. There is generally far less reaction than after ordinary amputations of the penis. In about a week the eschars begin to be detached in fragments, and they have all fallen in fifteen or twenty days. By this operation, then, we can much more readily act on healthy tissue, we are enabled to attack cases the knife could not reach, and we prevent hemorrhage.— *Gazette des Hôpitaux*, No. 18.

British and Foreign Medico-Chirurgical Review, July 1849, *p.* 292.

92.—*A New Operation to supersede Castration.*—By C. Taylor, Esq., Alfreton.—[Mr. Taylor having observed that the process of "sow-gelding" consists, not in the removal of the ovaries, but in the simple division of the fallopian tubes, advises a parallel operation, viz. the division of the vas deferens, (or the removal of a portion of it), to supersede castration in the male. He states that he has performed this operation successfully upon a dog.]

Lancet, Oct. 27, 1849, *p.* 447.

93.—*Remarks upon the Origin of Venereal Disease.*—By F. C. Skey, Esq., F.R.S.—[Mr. Skey thinks that a great many of the prevailing notions upon the subject of the venereal disease are erroneous; and, among the rest, those usually attributed to Mr. Hunter. He finds that ninety-nine out of one hundred venereal sores are better cured without mercury than with it. Amongst those forms of sore which Mr. Skey cannot cure without mercury, is one characterised by intense induration, to which induration, ulceration succeeds; this form of chancre, is, however, according to Mr. Skey's experience, extremely rare. Mr. S. goes on to say:]

The first principle I wish to establish is this, that the very large majority of venereal sores are what may be termed *simple sores*, and that such are the effects of a poison *of the least active and virulent kind.* That the poison becomes early exhausted in the progress of the sore, which assumes a simple, inoffensive form as soon as the first stage, or that of ulceration, has passed away. This sore presents, however, peculiar characteristics throughout its progress, which alone distinguish it from ordinary sores.

At present I wish to say a few words on the origin of venereal sores. The opinion almost invariably prevails, when a patient exhibits a primary sore on the genital organs, that that identical sore has been obtained from the infected source. I believe there is no opinion more erroneous. *Venereal sores are*, for the most part, *self-generated* in the male. A sore may become the product of any form of irritating secretion in the female, as may gonorrhœa also. This is an opinion I have long entertained, and every year's experience more strongly confirms my conviction of its truth. I am prepared to affirm my belief that, having intercourse with a woman without venereal disease, a man shall acquire one or more venereal sores, of one or other character, or even gonorrhœa. Unless this doctrine be founded in truth, it would be difficult to explain the existence of venereal disease at the present day; but I am satisfied that it is founded in truth, and a most important doctrine it is. The records of surgery, if consulted, clearly point to the fact; I appeal to the writings of Abernethy, Rose, and Evans, and others. Mr. A. gives at length the case of a gentleman, who having visited the mistress of his friend, got a bad venereal sore, on three several occasions. All this time, a period of nearly three years, this woman was domesticated with the man who kept her, and was never suspected by him to be the object of venereal disease. Mr. Rose tells the well-

known story of a Portuguese opera dancer who, undisturbed in her professional avocations, gave venereal disease of a formidable kind to a host of British officers during the Peninsular war, who had connexion with her. Mr. Evans, from whose excellent work on venereal disease I also quote from memory, relates the fact, that while the army of occupation resided at Valenciennes, he had more than 100 soldiers in the hospital from venereal causes, and that the usual examination of the women of the town, at the moment exhibited but three examples of the disease among the entire sisterhood.

Within the last three months, a medical man, who consulted me relative to a sore, volunteered the opinion, that the woman in question, could not be the subject of disease. Three several times did he examine her, and detected nothing. A patient in St. Bartholomew's Hospital, the subject of rheumatic gonorrhœa and articular rheumatism, declared that he had got the disease from his wife, and that he would drive her out of the house on his recovery. I asked him, "Have you any other reason to suspect your wife's fidelity beyond your having discharge." He replied, "No." "Then let me examine her, for I dare say she has nothing amiss." I did so. I found nothing—no disease of any kind beyond what is commonly found—a slight degree of leucorrhœa. I have elsewhere recorded a case of severe gonorrhœa, contracted by a medical student who never had had intercourse but with one person, and that a young girl, living under her mother's roof, whom he seduced. On one occasion, by some accident, he had intercourse with her before her catamenia had ceased. But these cases are not necessarily confined to persons of the humbler station; a sprinkling of them exists throughout society.

A gentleman well and intimately known to me attained twenty-seven years of age without ever having contracted venereal disease. He moved in a circle which associated him with women of rank and station. He accomplished an interview with a young lady during the absence of her family, who *at the last* resisted him successfully. He showed me, within a few hours of the transaction, a large excoriated surface on the inner side of the prepuce and glans, the result of violence. On the day following, he had intercourse with a married woman in the same rank of life, having all the external indications of perfect health. I asked him whether, as a man of the world, he had the smallest reason to think the latter lady could be the subject of disease. He thought it quite impossible. Now, in truth, it is a good way from impossible, but it was highly *improbable*. So far as he could make inquiry he did so, and the inquiry confirmed his opinion.

The result of the second act was increased inflammation of the already abraded surface, and excessive pain. Early on the following morning, he called on me, and, among other questions, asked me whether I considered him to have the pox? and especially, whether he should have secondary eruptions? I answered unhesitatingly,—No. The wound, however, became phagedænic, and

sloughed, and I applied nitric acid, and it sloughed again; and, in consultation with Mr. Welbank, strong nitric acid was resorted to. A large slough came away, and exposed a sore of formidable dimensions; and while this sore was in progress of healing, viz., in six weeks from the date of the poison, this gentleman was rewarded by an *intense phagedœnic eruption from head to foot.* He was now subjected to repeated salivations by mercury, having placed himself under the charge of another surgeon, and was twelve months under treatment. Now, in reasoning on this and similar cases,—for they all carry the same evidence,—I would ask, Is it probable, or even possible, that these women have disease without being conscious of it? In the case quoted from Mr. Abernethy's work on "Diseases resembling syphilis," is it possible that this woman had sores, or even discharge, for three years without being conscious of it. Is it reasonable to imagine that three women, in the cases reported by Mr. Evans, an acute observer, truthful in his relation of facts, and ingenuous in his reasonings upon them, could have communicated disease to 100 men; or, in the case I have last related, is it more probable that disease existed in this lady unsuspected of herself, or of another, who would have contracted it. It seems to me a just inference, that if a woman is unconscious of disease, having all the attributes of health, her husband being equally free, that the woman has not disease; and I believe such a woman, and in this condition is competent to give venereal disease to a third person having intercourse with her, either through her own secretions possibly existing at the time in excess, or through the remains of catamenial discharge. At all events, that the secretions of the two persons combined can produce and develope a *tertium quid*, which may have all the attributes and properties of venereal poison—a poison more readily developed in the person of a man having occasional than having frequent intercourse with the woman in question. Of course, I do not dispute the fact that disease really existing in either sex may be readily communicated from one person to another, though I doubt its being necessarily developed in the same form. Though gonorrhœa will generally produce gonorrhœa, the character of the sore is not by any means necessarily identical with that which produced it.

Now, in these opinions, I am happy to say I do not stand alone. I am persuaded that many thinking men, if they have not adopted them entirely, are prepared to extend their belief to the whole doctrine; and I do honestly believe that such convictions would become universal in the profession, were it possible to divest our minds of the impressions of early education.

Medical Times, June 2, 1849, p. 601.

DISEASES OF THE SKIN.

94.—ON THE LOCAL TREATMENT OF CHRONIC ECZEMA AND IMPETIGO.

By Dr. J. Hughes Bennett, Professor of the Institutes of Medecine, Edinburgh.

[In *acute* cases of these diseases, Dr. Bennett remarks, the ailment will generally be found associated either with a scrofulous taint, or with the oxalic, the lithic, or the phosphatic diathesis, and must be attacked by the constitutional treatment proper in these several instances; local treatment being of very subordinate importance. Of the *chronic* cases of impetigo and eczema, Dr. Bennett says,]

It is in cases of this kind that local applications are essential, and constitute the chief part of the treatment, whereas, in more acute cases, although they may be very useful, they are secondary in importance to internal remedies. The application I have employed for the last nine years has been a solution of ʒ ij. of the subcarbonate of soda, in a pint and a half of water. But an alkaline lotion in itself, applied from time to time, as is most commonly done, will not prove of any great service. I have found it necessary to place lint saturated with the solution over the affected parts, and to cover the whole with oil-silk, in order to prevent evaporation. Keeping the surface moist seems to me a necessary part of the treatment. The usual effect is soon to remove all local irritation, and especially the itching or smarting so distressing to the patient; to keep the surface clean and prevent the accumulation of those scabs and crusts, which in themselves often tend to keep up the disease. After a time, even the indurated parts begin to soften, the margins of the eruption lose their fiery red colour, and merge into that of the healthy skin, and finally the whole surface assumes its normal character.

I am quite satisfied that this local treatment is far superior to the stimulating ointments and lotions, which are so frequently used one after the other, in the hope of overcoming these obstinate forms of eruption. Many of them, indeed, only seem to augment the disorder. In these cases there is an increased exudation from the skin, not only of sebaceous but of purulent matters. Alkalies, we know, have the property of dissolving these, and acting as a calmative and emollient to the irritated part. Their constant action on the skin, therefore, in the manner I have described, may easily be imagined to be productive of benefit. But if, as is usually done, alkaline lotions and baths are only employed from time to time, they remove the sebaceous matter, leave the skin dry and harsh, and thus these applications often irritate. Keeping the surface covered with moist lint on the other hand, while it serves to protect, removes the results of exudation and prevents incrustation.

I have now treated a great number of persons affected with chronic eczema and impetigo, and have succeeded in curing them in this way, after almost every other kind of local application had been tried in vain.

Monthly Journal, Aug, 1849, *p.* 963

95.—*On the Treatment of Eczema.*—By Dr. T. H. Burgess.—[Speaking first of *eczema capitis*, Dr. Burgess reminds us of several circumstances which should be borne in mind in the treatment of all eruptions of the scalp in children. First, he says, we should remember the different forms of eruption peculiar to robust and scrofulous children respectively, as, owing to the state of the general health very different treatment may be required. Secondly, in inflammatory eruptions, we should not use stimulants except in the most chronic stages. Thirdly, when there is much cutaneous discharge, we should be very careful not to suppress it too suddenly. Fourthly, in all affections of the scalp, the hair should not be shaved, but closely cut. And lastly, as a general rule, all greasy applications should be abandoned. As regards the disease under consideration, *eczema capitis*, the treatment, especially when the affection is chronic, should be principally constitutional, and should consist of a course of mild tonics and alteratives. The local treatment, in the first instance consists in the usual measures of cleanliness. Dr. Burgess tells us:]

When circumstances are favourable, and the proper time arrives for arresting the discharge, it should be effected in as gentle a manner as possible, so as not to derange the internal organs; and for this purpose I prefer mild alkaline lotions of the carbonate or bicarbonate of potass, and the frequent use of the simple or emolient bath, in which the patient, unless very feeble, should remain at least one hour: the usual method of keeping the patient in twenty minutes or half an hour, being useless.

If there is an abundant serous exudation, and much smarting in the parts, half a drachm of sulphuric acid in a pint of barley-water will be found very useful. The patient should commence with small doses, and take a little cold water after each dose, till the stomach becomes accustomed to the acidulated drink. If the discharge continues undiminished, and the eruption does not appear to be affected by those remedies, we must have recourse to alteratives, as sarsaparilla and hydriodate of potash, to active purgatives, if the patient is strong, and to lotions of the nitrate of silver, or of the bichloride of mercury. If there is inflammatory tendency in the parts, the application of a few leeches behind the ears will be necessary.

Chronic eczema of the legs in adults, and especially when the patients are of a debilitated or broken down constitution, are generally very difficult and unsatisfactory cases to deal with. The practitioner seldom sees the case until the disease is so fully established, and, as it were, ingrafted on the limb, that the habitual condition of the parts seems to be that of subacute erysipelas,

with puffiness of the ankles, and a swollen if not varicose state of the veins; all denoting a sluggish or impeded circulation in the extremity. This state of things not infrequently terminates in that form of indolent ulcer of the legs so familiar to practitioners, and so difficult of cure.

For cases of this description I have found the application of the vapours of sulphur and iodine, in combination, the most certain and effectual remedy, when assisted during the interim of the applications, by bandaging the limb from the foot upwards.

My attention was directed several years ago, by Mr. Alfred Walker, to a preparation of sulphur and iodine, the vapour of which is an admirable local application in several cutaneous diseases. At that period I witnessed its marked benefit in many cases of chronic ulcer, under Mr. Walker's care. These remedies combined seemed to possess healing properties which are not manifested when they are used separately. When employed judiciously, and in appropriate cases, they appear to alter the vitality of the morbid parts, and to induce a state of healthier action. If the eruption is indolent, they gently stimulate the diseased surface into greater activity; and by regulating the strength of the remedy, according to the nature of the case, and the object in view, the most salutary effects may often be derived from its use. I have seen cases of lepra of several years' standing, which had resisted every other treatment, cured in a very few months by the application of the vapour of sulphur and iodine. It is particularly applicable to squamous and tubercular diseases of the extremity, to chronic eczema, and ill-conditioned ulcers of long standing. It may be administered in this form—℞. Sulphuris, ʒiij.; hyd. sulph. rubri, ℈ij.; iodinii, gr. x. M. fiant pulv. sex.

One of the powders to be used in the following manner three times a day. If the disease be seated on the limbs, a tin case or even a common jar, which will answer as well, provided it be large enough to hold the limb, should be procured; a heated iron is to be placed at the bottom of this apparatus, with a grating above it to protect the foot or hand. One of the powders being placed on this heated iron, the limb is to be instantly put into the bath, the mouth of which should be covered over to prevent the vapour from escaping. The limb may be continued in the bath for from fifteen to twenty minutes, according to circumstances. In the course of a day or two the proportion of iodine may be increased; for example, thirty grains of iodine, and at a later period double the quantity, may be incorporated with an ounce and half of the flowers of sulphur, to be divided into twelve powders, and used in the same manner as the former. I do not mean to extol this agent as a specific for the cutaneous eruptions indicated above; nevertheless, from what I observed both in Mr. Walker's practice, with reference to ulcers, and in my own with regard to skin diseases, I think it is worthy the attention of practitioners, and that it will be found a very efficacious remedy.

Medical Gazette, June 1, 1849, *p.* 934.

96.—*On the Nature and Treatment of Acne.*—By Dr. T. H. Burgess.—[Dr. Burgess makes the following interesting observations upon this very annoying affection:]

Acne is the result of chronic inflammation of the sebaceous glands, in which the hair follicles are more or less involved. The sebaceous matter becomes so inspissated that it cannot be discharged. It necessarily obstructs the follicle: inflammation of this follicle ensues, by which it is ultimately destroyed; a pimple is formed, and is soon followed by the formation of matter. One of the leading characteristics of this malady is, the inspissated condition of the sebaceous matter. This abnormal alteration takes place, not in the follicle by which the matter is discharged, but in the gland where it is secreted; and when we bear in mind the intimate association that exists between the hair follicles and sebaceous glands, it is not difficult to see that inflammation of the latter must necessarily involve the former, more or less: that the lesion originates in the sebaceous glands, and is subsequently extended to the follicles—both being invariably involved—is a position, I believe, which is now fully established.

Acne punctata derives its name and character from a small black spot which may be seen on the summit of each pimple. The indurated deposit is the result of follicular hypersecretion, and the characteristic black spots are simply the effects of exposure of the sebaceous matter at the mouth of the follicles to the carbonaceous matter contained in the atmosphere. These discolourations are most numerous on the cheeks, especially on the alæ of the nose, and can be felt by passing the finger gently over the parts. It was in the black indurations that M. Simon announced the existence of parasites; but the severest form of the disease is

Acne rosacea—the *couperose* of French writers. This variety differs in its anatomical element from the preceding, inasmuch as the disease involves clusters of follicles instead of individual ones, and at a later stage depôts of matter occupy the place of these disorganized follicles. Acne rosacea occurs most frequently in persons of mature years, whereas the simple punctata are commonly met with in youth, especially in females, producing great disfiguration of the face. It usually appears in the form of a number of red spots on the nose and cheeks: these spots are accompanied by a sensation of heat and tension, which gradually increases under the influence of certain kinds of food of high temperature, and of vinous and spirituous drinks. Pustules soon form at the summits of the morbid eminences. The parts affected become the seat of a kind of chronic inflammation; the skin is swollen and injected, and assumes a violet tint; the superficial veins are often dilated and varicose, and when all those morbid alterations are present at the same time they give the countenance an extremely disagreeable and repulsive appearance. The disease may assume a still severer form

than this: in the event of which the inflammation extends more deeply into the cutaneous structure; the pimples are more numerous; the pustules are frequently renewed; the process of suppuration is imperfectly performed, and the prolonged inflammatory action produces a degree of induration in the sebaceous follicles, forming true cutaneous tubercles. The cellular tissue now becomes involved, and the variety called *acne indurata* is the result.

Causes.—The causes of this eruption are various, and depend very much on the period of life at which it occurs, on certain peculiarities of constitution, as well as on the general state of health. For example, *acne punctata* almost invariably attacks young and healthy persons; whilst *acne rosacea* in the vast majority of cases attacks individuals advanced in years—more especially females at the critical period. As a general rule, acne occurs in females more frequently than in males, and this peculiarity is intimately associated with the condition of the uterine function. The simple form of the eruption frequently attacks young girls at the period of the first menstruation; whereas, as already stated, the severest variety—acne rosacea—is usually met with in women at the turn of life. Besides, when the menses are suppressed between these periods, or materially deranged, one or other of the varieties of acne is almost certain of making its appearance. Acne rosacea may also depend on other causes; as, for example, gastric or hepatic disorder, high living, violent mental emotion, the use of cosmetics or irritating lotions, &c. It is sometimes hereditary. It is worthy of remark, that when acne occurs in youth, it generally appears in individuals of sanguineous temperament, whilst it commonly attacks persons of a bilious habit of body when it occurs later in life. Some persons are predisposed to the disease from certain peculiarities in the original formation of the skin, and the size and disposition of the follicles about the face, especially the variety called rosacea. In such cases disorder of the chylopoietic viscera, or of the uterine functions, will be sure to bring on an attack of acne.

Treatment.—The first points to be inquired into before commencing the treatment of acne, are the state of the functions, and the general condition of the system. Acne being a mixed disease, depending partly on local peculiarity in the original formation of the skin, and partly on the functional disturbance, it is obvious that unless we take into account at the commencement, the etiology of the complaint, we cannot possibly lay down a rational method of cure. An exclusively local treatment would be as unavailing as empirical; and to confine ourselves to constitutional remedies alone would be equally irrational and erroneous. The application of those general symptoms referred to in the introductory remarks is specially indicated in the treatment of acne; and it is only by a due consideration of them that an effective method of cure can be prescribed.

Medical Gazette, Aug. 24, 1849, *p.* 314.

97.—*Cases of Psoriasis treated by Arsenic.*—By S. Griffiths, Esq., late House-Physician to King's College Hospital.—[Mr. Griffiths gives several cases of psoriasis treated by arsenic, as confirmatory of Mr. Hunt's views in regard to the efficacy of this remedy. The first case was that of a boy aged nine years; liquor potassæ, nitro-muriatic acid, &c., had been given internally, and various local applications used, but only with temporary benefit, the disease soon returning. The following report is given:]

May 29th, 1847.—At present there are numerous small patches about the trunk; others on the arms, spreading, as it were from the elbow-joints on either surface; the largest are scattered on the anterior part of the knees and legs; the patches are much raised, scaly, and itchy, which induces the boy to scratch, when an ichorous fluid is discharged, forming a hard cake over several of them. The other functions appear healthy. Ordered, Fowler's solution of arsenic, a drachm; distilled water, seven drachms. Mix. Forty minims to be taken, in water, thrice daily.

June 19th.—A marked change in the character of the eruption has taken place; it is paler, less scaly, and there is a total absence of the irritation which so much annoyed him a fortnight since. No fresh patches have appeared, nor are there any unfavourable symptoms from the use of the arsenic; the only indications of the system being under its influence are the amendment of the disorder, and an almost imperceptible inflammation of the conjunctiva.

He continued the same treatment up to July 15th, when all trace of the disease, with the exception of a slight discoloration of the skin, had disappeared.

Feb. 22nd, 1848.—No return of the eruption; the skin is quite free from spots or roughness.

March 30th.—Two small spots have shewn themselves on the right knee, he recommenced taking the drops a fortnight since. The spots are now almost gone. To continue the arsenic, in four minim doses, for a month longer.

Feb. 1849.—The boy is well, and entirely free from his former complaint, of which he has had no return.

[The next case was a very inveterate one, occurring in a woman 47 years old.]

Dec. 16th, 1847.—The state of the digestive organs having been rectified by small doses of blue pill and colocynth, five minims of Fowler's solution were given with the food three times a day; the sores dressed with spermaccti cerate and bandaged; to have plain but nutritious diet.

Jan. 14th, 1848.—The eruption is already presenting signs of amendment, being less irritable and scaly; the conjunctiva is affected, and she has a slight attack of gastritis, to which she has always been subject on the disappearance of the eruption, though never when it was fully out. Thinks herself stronger; appetite indifferent; tongue clean; bowels regular. Continue the arsenic.

Feb. 12th.—Since the last report, there has been a much more marked improvement. The eruption generally is fading, and does not cause so much annoyance; there are no fresh patches; the varicose veins on the legs are better; she has more appetite; no return of gastritis; conjunctiva is still injected, but not sufficiently so to cause annoyance.

March 15th.—She feels herself much better; the scaliness and prominence of the disease has left in many places; she can now rest comfortably, as she feels no inclination to scratch; conjunctiva still affected; she has no gastritis; the tongue is clean; bowels regular; appetite good.

April 16th.—The whole diseased surface is free from scaliness; the ulcers on the legs have been for some time healed; the varicose veins are less distended; and she can wear her shoes, which she has been unable to do for some time previously. Repeat the medicine.

June 14th.—Health appears completely restored; the only trace of eruption is the brown state of the skin, which otherwise seems to be quite sound.

The arsenic was continued, in four minim doses, for three months longer, when the discoloration of the skin had disappeared.

December.—No reappearance of the disease.

I cannot forbear offering a few observations on some of the principal points in the cases above related, especially with regard to the mode of administering arsenic. This medicine, prescribed by others, has frequently been productive of great benefit in the cure of chronic skin diseases, but no author is to be found who has met with such marked success in its use as Mr. Hunt. At its first introduction, as indeed often happens with other agents, its influence was decided in the hands of many. Not so in later cases; and many practitioners have, by degrees, come to lose all faith in it. This may, I think, be readily accounted for. A man unaccustomed to arsenic would be very cautious in prescribing it, and this very fear would lead him to use it in something like the manner in which its beneficial effects are most apparent. As, however, his confidence in it grows stronger, and his fear of its baneful effects subsides, he is induced to give it in larger doses, hoping more quickly to eradicate the disease; this not only interferes with its proper action, but also causes it injuriously to affect the system.

Arsenic should never be given when there is any inflammatory action about the system, otherwise this state is sure to be increased; all the functions must first be brought into a healthy condition.

Whatever be its *modus operandi* in the cure of skin disease, my experience goes decidedly to establish the tonic action of arsenic. Under its use alone, and without any alteration in diet or residence, I have seen marked improvement, not only in the disease, but in the general health of the patient; these, in fact, seem usually to go hand in hand. Herein is shown its specific action, as distinguished from other tonics; these, when exhibited, cause improvement of the health, but exert no apparent influence on the eruption. I am aware that in this opinion I differ from one of our chief authorities,

Dr. Pereira, who, though he has tried it extensively, and with his attention directed to this point. has been unable to satisfy himself of its right to be classed as a tonic; the appetite of his patients sometimes improved while taking it, in no other respect was its tonic action shown. Vogl, (Pharmakodyn,) and many others, have arrived at the opposite result.

And here I would advert to the common mode of giving arsenic —i.e., in increasing doses. What tonic could be given in such a manner without danger of producing severe constitutional disorder? It is the peculiarity of this class of medicines, that their action is so gradual as to be almost imperceptible, days, or perhaps weeks, being required to show any decided effect. Thus is it with arsenic; though its action is more manifest from the complaint being external. Arsenic ought, therefore, to be given in moderate, or what might be called tonic doses, at first, until the system comes under its influence; these must be diminished, if it affects the patient too severely. The action of arsenic seems to be more direct on the tegumentary tissues, the skin, and mucous membranes, and its influence is nearly always first manifested on the tarsal conjunctiva; hence we have a ready guide to its exhibition. The degree at which we wish to arrive, is a very slight injection of this membrane; if it goes beyond that point, and more severe irritation is caused, then the eruption becomes inflamed, and even spreads; this may in almost every case be prevented by diminishing the dose. If the system be kept in this state, no injurious operation need be feared. Five minim doses of Fowler's solution may be safely given at first to a person who is moderately strong, has not had his constitution injured by its abuse, and who is not unusually intolerant of it. Cases will, however, present themselves to the practitioner, in which it will be advisable to begin with a smaller dose than this; indeed, if there be any doubt, it is better to give the smaller dose, two, three, or four minims, and pursue it for a longer time; the system is sure, though at a later period, to come under its power. The disease is generally seen to date its improvement from the time of the first affection of the conjunctiva.

Again, arsenic should be taken with the meals, or on a full stomach; this serves to dilute it, and prevents the gastric irritation which so frequently follows when it is exhibited without this precaution.

The decided influence which this remedy has over certain eruptive disorders, is shown, not merely by their amelioration under its proper use, but more manifestly by omitting it for a time, as I have frequently done, and invariably has the disease recommenced spreading as soon as the system had got rid of the arsenic, as shown by the subsidence of the conjunctivitis, and its course has been again stayed when the medicine has been resumed sufficiently long.

The rule which Mr. Hunt has laid down in his very excellent work—viz., "the arsenical course should be protracted (in reduced doses) as many months after the final disappearance of the disease,

as it had existed years before." Though perhaps not necessary in every case, it is always advisable, as at this time it is much less difficult to eradicate the tendency to the disease, than, on its reappearance, to travel again over the same ground.

In conclusion I would observe, that though, as is well known, injurious effects have followed the injudicious administration of arsenic, in no one case have I seen the constitution injured, or the health impaired, when it has been exhibited in the method here advocated; on the contrary, the health has improved, the disease disappeared, and has not returned, when the arsenic was persevered in as long as was deemed necessary.

Lancet, May 26, 1849, *p.* 559.

98.—*Case of Lepra treated by Arsenic.*—By Samuel Griffith, Esq., Late House Physician to King's College Hospital.—[The patient was a stout boy twelve years old, who had first perceived the eruption (mixed psoriasis and lepra) about two months before applying for relief. The following is the account of the treatment:]

Dec. 28th, 1846.—There being nothing to contraindicate its use, Fowler's solution of arsenic was given, in doses of five minims, with his meals, three times a day.

Jan. 6th, 1847.—The psoriasis continues about the same, but the body is nearly covered with a papular eruption, (lichen simplex.) The itching is troublesome, especially in the night, preventing sleep. The conjunctiva is affected. Continue the arsenic.

16th.—The lichen has died away, leaving the skin coated with fine scurf. The lepra is fading, no fresh patches have appeared; the sub-maxillary glands are swollen and tender, and he complains of weakness of the eyes. The conjunctiva being more injected than is consistent with the proper action of the medicine, the dose was reduced to four minims.

30th.—The eruption is fast fading; the swelling of the glands has subsided, but the conjunctiva remains affected. The general health continues very good. Skin free from the lichenous scurf.

Feb. 15.—The lepra is quite gone, but has left the usual brown stains. The arsenic was continued to the end of March, when the skin had resumed its natural appearance. There had been no return of the eruption when I saw the patient four months subsequently.

Lancet, May 26, 1849, *p.* 559.

99.—*On the Treatment of Lepra and Psoriasis.*—By M. Emery.—M. Emery states, that when appointed to the St. Louis, several year ago, he tried all the various remedies so warmly recommended by authors, and of all these *arsenic* proved the best internal medicine; but besides the inconveniences which it sometimes gave rise to, its operation was very slow. The external use of *strong tar*

ointment ($\frac{1}{3}$ to $\frac{1}{4}$ of tar) produces, in fact, a far more rapid cure than any other means. Of from 1500 to 1800 patients who have employed it, five sixths have been rapidly cured, and that without any ill consequence, or any greater frequency of relapse than after internal means. In 228 cases of psoriasis, the arsenic and tar were used conjointly, and 200 were cured within two months. Of all the preparations of *mercury* the *protiodide* is alone efficacious ($\frac{1}{8}$); but it excites much irritation of the skin, and if applied to large surfaces, produces salivation. The *iodide of sulphur* is useful in psoriasis of the head; but if applied to large surfaces, produces erysipelas. The conjoined use of the arsenic and tar-ointment constitutes, in fact, the best medication. In a disease so apt to recur, the greatest attention to diet is essential; and on the least symptom of recurrence, resort should be had to medicine, without waiting until it has become very bad. Before commencing, the patient takes a bath, and rubs in the ointment with gentle friction three times a day. In two or three days the strength of the ointment and activity of the friction are increased; and when the disease is of old standing, linen rags smeared with the ointment are to be kept applied. A tepid bath is taken once or twice a week. It is only in very irritable skins that the treatment is interrupted by the appearance of impetiginous pustules or furunculi. In about ten days we perceive, where the squamæ have fallen, a whitish circle circumscribing the patches, and extending from the circumference towards the centre. This is a sign of a decrease of the disease, which usually disappears in two or three months. With respect to the arsenic, we should begin with five drops of Fowler's solution, in four ounces of sugared water, which are to be divided into two doses. Every other day the dose is to be increased a drop, until twelve are reached. If we observe that the patches are becoming thinner, and acquiring a blackish-gray colour, this is a sign of saturation, and the dose is not to be increased; but even when this is not present, it is rarely proper to go beyond twelve or fifteen drops. Sometimes the skin becomes hot and painful around the patches; but this is relieved by tepid lotions, demulcent drinks, and diminution of the dose. After twelve or fifteen drops are reached, a feeling of constriction of the throat, or severe pain of the stomach, is perceived; and then the medicine should be suspended for a day or two, and recommenced *de novo* with the small dose. Pain near the heart, with palpitation, sometimes renders venesection requisite. If any contraction of the extensors of the limbs is observed, the medicine must be at once abandoned; and as soon as the blackish-gray colour of the patches appears, which announces saturation and an approaching cure (though such spots may remain for months), the arsenic must be discontinued.—*Bulletin de Thérapeutique*, tom. xxxvi, pp. 481-90.

British and Foreign Medico-Chirurgical Review, Oct. 1849, *p.* 554.

100.—*On a Means of rendering certain Substances Incombustible.*—By Dr. R. A. SMITH, Manchester.—[In seeking for a substance capable of preserving combustible articles from the action of fire, it appeared to Dr. Smith that:]

Sulphuric acid seemed to present the most promising characteristics of a substance incapable of burning, and of acting so strongly on vegetable substances as to make them incapable of burning. Sulphuric acid itself is a body perfectly burnt, or we may say overburnt, having an atom of oxygen given to it by artificial means, so to speak, which atom is difficult to separate, and therefore not resembling the oxygen of many highly oxidised bodies. It requires a high degree of heat to raise it to vapour; and the vapour formed is sluggish and heavy, remaining long where formed, and quenching flame wherever it is. It destroys the texture of wood also and other vegetable substances, causing them to give out after a time gases which do not burn, mixed with some which do burn; but if there be enough of acid, forming a mixture which does not burn. The wood also cannot be again induced to become combustible until it be heated to redness, so as to remove all the sulphuric acid, leaving only charcoal.

If sulphuric acid could be introduced into the wood just at the time that the fire was going to take place, the fire would cease to take place; and this we can do easily by saturating the wood with sulphate of ammonia. When there is no fire present, there is no sulphuric acid present, as such; but as soon as the heat rises, ammonia goes off, and sulphuric acid is instantly presented to the wood. The ammonia does not come off quite pure, it is mixed with nitrogen and sulphurous acid; and this disengagement of gases is of advantage in extinguishing fire; when the heat rises to 536° the sulphuric acid is then left to act on the wood in part and to volatilize in part, and that which I have mentioned takes place. The outside of course would first undergo the change, and the inside would be protected by the incombustible outer part; if the fire continued to act long, the inner layer would undergo a similar change. I imagine, then, that the acid acts in a double manner; it makes the wood refuse to burn, and puts out fire. As sulphurous acid is given off in this process, the action is also similar in one point of view to that of sulphur, which has long been used for fire in chimneys.

I have no doubt that a house built of wood prepared in this manner might have a fire lighted on the wooden floor without danger, burning only on the spot to which the fire was limited. A ship also would be safe, even if the cinders did fall from the grate in stormy weather.

I know that muriate of ammonia has been used, and that it acts very well; but I think the sulphuric acid is superior, the ammonia being only to keep it innocent; any other volatile base might do. I am sorry, however, that this is not perfect; its solubility in water is a great disadvantage, as it cannot be applied to clothes to be frequently washed. True, it is so cheap that it might be applied every

washing where there are peculiar dangers; but if a person were standing very near the fire, the ammonia would in part be evaporated, and the acid remaining would be enough to injure the fabric.

There are, however, cases, such as curtains, to which this could not apply, and where it would be valuable.

Sir William Burnet's liquid is chloride of zinc: he uses it for preserving wood and canvass, and also for preventing fire. I am certainly surprised that more use has not been made of it, being, as far as I have seen it, so efficient. I believe the manner in which the chloride of zinc acts is very similar to that of sulphuric acid, destroying the organic matter on the approach of heat, and rendering it incombustible. It can be introduced into wood at a specific gravity of 2000, I believe; sulphate of ammonia cannot easily be used above 1200. By heating the solution more may be attained. Sulphate of ammonia is cheap and easily procured and used, not hurting anything with which it may come in contact, and therefore more easily managed in households.

The chloride of zinc is said to unite with the fibre. This cannot be said for the sulphate of ammonia. It would not, however, come from the centre of a beam of wood, even if immersed in water, as the water enters with great difficulty into wood; and the solution itself cannot be introduced without forming a vacuum in the saturating vessel, and so removing all the air from the wood.

The first time I used this solution I found a large quantity of mould formed, and indeed it contains all the elements to increase its growth. The second time the solution was boiled in an iron vessel, and no mould formed on it; on the contrary mould was destroyed by it. The sulphate of ammonia dissolves iron rapidly, and forms a double salt which is deleterious to such growths. I imagined any other metallic salt would do, and used ordinary chloride of manganese prepared in the laboratory, which killed all such fungi rapidly, and no more have grown after standing eleven months in contact with organic matter.

I believe there are many ways in which this may be used. My wish was to find a substance suited for building fire-proof ships, and I believe this would do; at any rate the ships would be fire-proof, experience could alone tell if any other objection followed. It does not render the wood hard, heavy, or brittle.

I believe it would be of the greatest advantage in mills, which now suffer so much from fire, diminishing or rather entirely removing the expense of insurance. It does not hurt colours; so that even coloured goods might be dipped when kept long in one place, or when sent in vessels abroad. Possibly some delicate colours may be attacked, but this must be a rare case.

I am more desirous of seeing ships built of an incombustible material, the means of escape at sea being few and confined to few; and whilst there is any hope of doing it easily, I scarcely think it proper for any one to neglect what information may exist on the subject.

Provincial Medical and Surgical Journal, June 27, 1849, *p.* 351.

[At the Worcester meeting of the Provincial Association, Mr. Crompton, of Manchester, exhibited the action of the sulphate of ammonia in preserving paper, &c., from the action of fire.]

He exposed a piece of newspaper, which had been soaked in sulphate of ammonia to the flame of a candle—though the texture of the newspaper was destroyed, it did not take fire. The heat caused the ammonia to fly off, and the sulphuric acid being set free, charred the paper. As the sulphate of ammonia did not the slightest injury to the texture of the fabric, it was most desirable as a non-combustible application for children's dresses, bed curtains, &c.

Provincial Medical and Surgical Journal, Aug. 8, 1849, *p.* 431.

101.—*On the Treatment of Chilblains.*—By M. Ossieur.—In the earliest stage, friction, either employed dry or with brandy or sp. camphor, is the simplest and best means; but when the parts have become red, swollen, shining, and even covered with phlyctenæ, but prior to ulceration, the formula recommended by M. Goffin may be use with the greatest advantage:

Camphor, 4 parts; ess. oil turpentine, 30 parts.

When the practitioner is only consulted after ulceration has for some time taken place, M. Devergie's ointment is then the best application:

Lard. 1 oz.; liq. plumb. subac., 12 drops; thebaic extract, 3 grains; creosote, 10 drops.

—*Bulletin de Thérapeutique.*

British and Foreign Medico-Chirurgical Review, Oct. 1849, *p.* 566.

102.—*On the Treatment of Ulcers by Means of Opium.*—By F. C. Skey, Esq., Surgeon to St. Bartholomew's Hospital.—In the summer of last year Mr. P., a gentleman residing in Montagu-square, a governor of St. Bartholomew's Hospital, placed himself under my charge, with a large chronic ulcer on the calf of the right leg. Its margin was irregular and jagged, and was elevated to at least one-third of an inch above the base of the ulcer, which base was pale and bloodless. This wound was about the size of a man's extended hand, including the fingers, or even larger. It had existed for, I think, seventeen years. All remedies had failed, and the cure had long been abandoned as hopeless.

I gave Mr. P. half a grain of opium night and morning. In three days he himself noticed the singular red colour of the entire margin of the ulcer, which in a week extended over the base. I rolled and strapped the limb; the entire sore consequent on its depressed surface being placed beyond the influence of the pressure.

Within two months that sore was reduced *to the size of half-a-crown*, and the *only remedy employed was half a grain of opium night and morning*, which, by-the-bye, had a very beneficial influence on his health.

Medical Times, July 21, 1849, *p.* 61.

103.—*Use of Collodion in Bed-sores.*—Air-cushions and water-beds are not obtainable by the poor, and it has been the practice to protect parts subjected to injurious pressure either by powdering them with some inert substance, or covering them with diachylon. The first of these measures is of little use, and the other does more harm than good, as by softening the tissues to which it is applied, it only renders them more susceptible to the influence of pressure, especially if the leather on which it is spread gets into folds. Collodion adheres far more closely than diachylon, and is quite exempt from moisture; and in the cases in which it has been tried in the French hospitals, has answered well.

British and Foreign Medico-Chirurgical Review, July 1849, *p.* 299.

104.—ON TUMOURS.

By Bransby B. Cooper, Esq., F.R.S.

[Mr. Cooper would classify tumours according to the tissues from whence they arise, rather than according to their own physical conditions, since, he says, the effused mass resembles more or less the structures in which it originates, but existing under an altered and abnormal form. He observes:]

Mr. Abernethy has attempted a classification of tumours according to their internal structure and organization, placing them under the general heads of sarcoma and cancer, dividing the former into adipose, pancreatic, mammary, and tuberculated, and the latter into scirrhus and fungus hæmatodes, or, as he sometimes termed it, medullary sarcoma. It appears to me, however, that it would be more rational to classify tumours according to the structures whence they take their rise; and, indeed, this arrangement has already been adopted by Dr. Warren, of Boston, in his comprehensive work on this subject.

[After a few observations on warts,—tumours of the cuticle,—Mr. Cooper proceeds to speak of]

Encysted tumours.—Sebaceous follicles constitute a considerable proportion of the structure of the cutis, their function being to secrete a fluid for the lubrication of the surface of the skin: they may be considered, in fact, as the most simple kind of gland, and are composed of a small sac projecting internally into the cellular tissue, and deriving its blood-vessels from the capillaries of that structure. If from any cause the orifice of the follicle becomes closed, a retention of its secretion must necessarily follow, and this exciting a morbid action, alters the character of the secretion, and is converted into an encysted tumour. These tumours sometimes increase to a considerable size, but more frequently remain about the size of a pea, not appearing to possess any tendency to enlargement, unless subjected to irritation, when they often begin rapidly

to increase, and require to be removed by operation. The formation of an encysted tumour may proceed not only from the accidental closing up of the excretory orifice of a follicle, but also in consequence of inflammatory action which may induce a change in the character of the secretion, thickening it, and rendering it incapable of flowing from the follicle in the normal manner. Acne punctata may be considered as the simplest form of follicular encysted tumour.

In many persons there appears to be a peculiar tendency to the formation of encysted tumours, especially in the scalp.

Tumours in the cellular membrane.—Steatomata or fatty tumours are the most frequent morbid growths that occur in this tissue, and which sometimes acquire so large a size, as to lead to the necessity for their extirpation. Some persons seem particularly prone to the formation of these tumours: this would lead to the belief that they depend upon some constitutional tendency rather than upon any accidental local cause, as I have never been able to trace the formation of a steatoma to external injury. The formation of fat, moreover, always seems to arise from a kind of degeneration of tissue, and on that account must be regarded as depending as much upon an altered condition of the constitution as upon mere local action. These adipose tumours are very slow in their growth, causing little or no pain, even when they have acquired a very considerable size. Their physical condition is recognizable by their doughy feel, lobulated form, great mobility, close proximity to the skin, and from the skin itself retaining its normal appearance. If the tumour be pressed so as to tighten the skin, its lobulated form becomes quite distinct, and the freedom from pain during this manipulation constitutes a further diagnostic mark. In some patients several of these fatty tumours are found existing in different parts of the body at the same time; and I had lately a female patient in the hospital who had eight, three of which weighed at least three pounds each: one of them I removed from the upper arm in consequence of the inconvenience it caused her.

Steatomata consist of masses of fat enclosed in adipose membrane, and generally when they acquire any size they produce a thickening of the surrounding cellular tissue, which forms a cyst of greater or less density around the whole swelling; the lobulated form probably results from the distension of the cells of the adipose membrane, which are sometimes so distinctly separated as to give to the tumour when removed somewhat the appearance of a bunch of grapes. If the fat be soft in consistence, it is more diffused, and its lobulated appearance much less distinct; and I have met with steatomata, the contents of which were so soft as to give almost the sensation of fluctuation, and are liable to be mistaken for abscess. When the fat is deposited immediately beneath the skin and subjected to much pressure, the lobes are much smaller, and even sometimes granular, and the cysts proportionally thicker and stronger: such steatomata are more firmly fixed, and consequently removed with more difficulty when their extirpation is had recourse

to. Sometimes steatomata assume a pendulous form, adhering by a narrow neck as if a few of the adipose cells only were subject to the abnormal growth of fat, the tumour extending itself outwards, as if the enclosing cyst were capable of generating fat in the direction of its length rather than its breadth, forming a long pendulous tumour with a narrow neck. In removing these tumours care must be taken not only to take away the external swelling, but also to extract the quantity of fat situated subcutaneously. It is not unfrequent in these cases to find rather a large branch of an artery running through the neck of the tumour into the cyst, by which the fat, in fact, becomes generated beyond the normal influence of the capillaries of the original adipose membrane. In the removal of steatomata of this form, more than usual hemorrhage may therefore be expected from the manner in which the cyst is nourished.

In the operation for the extirpation of steatomata a free incision should be made through the skin to expose the outer capsule of the tumour: this capsule is next to be cut through, when generally the mass of fat may be turned out of its cyst as readily as a kidney may be drawn from its capsule. It rarely happens that any difficulty occurs in checking the bleeding, but the wound should never be dressed until the hemorrhage has perfectly ceased: or the cavity becomes filled with blood, and the union of the wound by adhesion will certainly be prevented. When dressed, the edges of the wound should be closely adapted and retained in their position by one or two sutures: a compress of lint should then be laid on the cavity, and slight pressure maintained by bandages. It is not always, as I have already mentioned, that these tumours are so readily extirpated, for if by exposure to pressure or external violence they have been subjected to inflammation, they may adhere so firmly to their capsule as to require the dissection of the latter from the surrounding cellular connections: in this case the period required in the operation must be much protracted. I have never known the disease return after its extirpation, nor have I ever witnessed what is described by some surgeons—viz. that the stimulus of the operation leads to the formation of these tumours in other parts of the body, even where there had been no previous apparent tendency to their formation. Indeed, I do not know of any surgical operation which promises a more favourable result, and have never known any untoward symptom occur excepting when the size of the tumour renders so large an incision necessary that a comparative degree of constitutional irritation must naturally result. Erysipelas, however, sometimes supervenes, even in these minor operations.

Tumours in the muscular system are of very rare occurrence, and indeed the muscular system seems particularly free from disease of any kind, for even the abnormal contractions of cramp or tetanus, and the involuntary motion of the muscles in chorea, must rather be considered as the result of disease of the spinal nerves than of the muscular system itself. It is true that malignant disease, whether it commences in bone, periosteum, or cellular membrane,

may secondarily affect the muscular system, but primary malignant affection of this tissue I have never seen. The only abnormal growth I have witnessed in the muscles is one of a fatty character, and which, as it is invariably indicative of constitutional deterioration, requires rather medicinal than surgical treatment, and it has never fallen to my lot to remove any morbid growth from a muscle; my surgery not having extended further than the division of muscular tendon for the relief of some permanent contraction of a joint.

Medical Gazette, Aug. 31, 1849, *p.* 353.

105.—*A Substitute for Tents in Dressing Abscesses.*—By M. Nonat. —In place of introducing tents, which are not only painful, but give rise to other inconveniences, M. Nonat passes a crayon of nitrate of silver into the opening, to the depth of from ½ to 1 centimetre, withdrawing it immediately. The pain caused by this is trifling and evanescent, and the eschar produced prevents the union of the walls of the trunk of the abscess. It is in abscess of the breast that this procedure has been found especially useful; the deepest abscesses being thus brought to heal in seven or eight days. The cauterization has to be repeated every two or three days.—*Gazette des Hôpitaux*, No. 23.

British and Foreign Medico-Chirurgical Review, July 1849, *p.* 292.

106.—*Use of Iodine in Snake-bite.*—Dr. Whitmire, in the communication from which this is condensed, states that he has used the tincture of iodine in bites of the rattle-snake, viper, and copperhead, on both man and beast, with complete success. His manner of using it is to paint the part that is bitten, and so far as the swelling extends, with three or four coats of tinct. (pharmaceutical strength) twice daily; and should the swelling extend which it almost always does after the first application, if made any time soon after the infliction of the wound, it should be followed up with the paint. By the time the third application is made, the tumefaction will cease to extend, and three or four more applications will generally restore the limb, or part affected, to its natural state, save perhaps an obtuse sensibility to touch, owing probably to the cuticle being destroyed, and some soreness of the muscles, which will remain a longer or shorter period. A successful case is related in illustration of the efficacy of this mode of treatment.—*North Western Med. Jour. (Amer.)*

Dublin Medical Press, June 20, 1849, *p.* 393.

107.—*Assacou, a new Remedy for Elephantiasis.*—[*Assacou* is the native name of a tree (Hura Brasiliensis, *N. O. Euphorbiaceæ*) indigenous to the Brazils. An account of the employment of the juice of this tree in elephantiasis (lepra tuberculosa) has been transmitted

to the French Government from one of the French consuls in Brazil. We are told that:]

Martius, in his "Systema materiæ medicæ vegetabilis Brasiliensis," regards it as an anthelmintic. The beneficial effect of this remedy was strikingly manifested in a case at Santa Maria de Belem. A man, suffering under lepra, had three years previously fled from the central provinces, in order to avoid the usual confinement adopted towards lepers. He was still afflicted with the disease, and on the assacou juice being proposed to him as a remedy, knowing its poisonous character, he gladly availed himself thereof, rather with the hope of putting an end to his existence than of being cured of his leprosy. The effects were, however, so favourable that he not only no longer hesitated to return to his native village, but was eager to do so in order that he might impart the secret to others. The case was investigated by a medical commission, and experiments were made upon four other cases with considerable success; although it must be borne in mind that these apparent cures require the confirmation of time, as this disease is peculiarly prone to return, and a subsequent fatal termination is by no means unfrequent.

The mode of exhibition is by pills made of the extract; of the juice in doses of a few drops; and in baths made by boiling the bark. The decoction is made by half an ounce of bark to a pound of water, adding twelve drops of the juice of the tree. The bark of the tree contains the most active principles. The decoction applied to the skin produces an eryspelatous inflammation, and a pustular eruption. The natives employ the juice as a poison, and to this no antidote is known.

Messrs. Merat and Gibert conclude their report to the minister, by stating—1st, That assacou is a potent and poisonous remedy; requiring to be given with great caution; 2nd, that the reports of the Brazilian physicians regarding its efficacy in lepra, require confirmation, since they themselves admit that their observations have not been sufficiently numerous; 3rd, that if confirmed, the importation of the remedy is desirable.—*Journal de Pharmacie et de Chimie.*

Medical Gazette, Aug. 10, 1849, *p* 263.

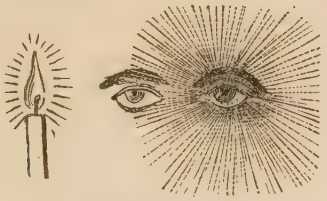
AFFECTIONS OF THE EYE AND EAR.

108.—ON A NEW MEANS OF ASCERTAINING THE EXISTENCE OF DISEASE OF THE EYES.

By J. D. Macdonald, Esq., Surgeon, R.N.

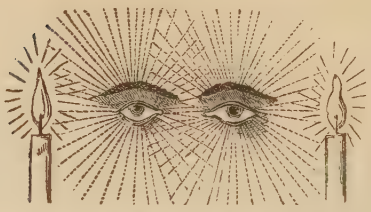
It is a fact but little observed, that when a stratum of dust is laid upon the surface of a mirror, each particle and its reflection so lie, one with respect to the other, that a line drawn through them both will be, in every case, as the radius of a circle, whose centre is in the pupil of one of the observer's eyes as seen in the glass; so that an appearance of rays is thus produced, seeming to emanate from that point. The matter in itself is trivial, but, from what follows, it will be found to afford a delicate test for discovering the relative strength or visual capacity of one eye, compared with the other, hitherto a desideratum in ophthalmic surgery.

If the right eye be illuminated by a candle while the left remains in shadow, the experimenter will perceive, by looking into a mirror, prepared as above, that the irradiation proceeds from the pupil of the shaded eye; and this without reference to its position.



Placing the light on the opposite side, (the left), the physical circumstances are altered, and the appearance is just the reverse of the former case.

But, should two candles be employed, one on either side of the observer's head, the lines formed by the dust particles, and their reflections, will either seem to irradiate from both eyes, as centres, or to spread from each side mutually across the opposite eye.



In explanation of these facts the writer finds, that when any circumstance incapacitates either eye from discharging its functions perfectly, (as the light in the first and second experiments cited) the unaffected organ appears to have dominion, and this is manifested by the radiation of the particles seeming to take place from its pupil in the mirror, and overpowering those of the other eye. In consequence of the sympathy existing between the optic nerve and iris, when a strong light falls upon the latter, the pupil diminishes in size, so as to regulate the amount of light impinging on the nerve, according to its sensibility. When one eye is thus influenced, its powers are lessened considerably, for, while it is directed to the image in the glass, the iris cannot admit a sufficient amount of light to impinge upon the retina from that quarter, having a much stronger stimulus in active operation to contend with from another. This state of things is quite reversed in a shaded eye, because the iris is free from the action of a powerful light, and has only to discharge its office in allowing the ingress of as many rays from the dust particles of their reflections as the delicacy of the optic nerve can bear, which fully accounts for the strength of the impression overpowering that of the weakened eye.

When two candles are employed, as in the third experiment, both eyes are equally influenced, receiving a similar distribution of light, and are, consequently, alike fitted *(cæteris paribus)* for the performance of their respective functions, so that the lines necessarily appear to irradiate from both eyes.

Now, the practical application of the experiments alluded to, (if properly conducted,) is this, that the least inequality of the powers of one eye when contrasted with those of the other, is instantly discovered, and the earliest onset of cataract, amaurosis, &c., is at once detected; for, if both eyes are similarly situated before a light which falls equally upon each, the patient will himself discover where the defect lies, independent of any other proof, agreeably to the explanations above given.

A very close relationship exists between the co-ordination of the muscular movements of the eye-balls and the function of adaptation to distance, and also an intimate connexion between this latter and the condition of the retina. Thus, if the retina of one eye be in any state of debility, the adaptive changes do not take place equally in both eyes, and, as a necessary result, the co-ordination

of the muscular actions which so wonderfully effects the concentaneous movements of the eye-balls is disarranged, and strabismus (or squinting,) is, under such circumstances, satisfactorily accounted for. If, then, in consequence of debility of one of the retinæ, whether from disease (as is usually the case,) or from whatever cause, there is a tendency to squint, the eye so disposed may be instantly detected by the foregoing experiment, in which it also assumes its wonted position. This test has been successfully tried by the writer in the case of an individual who had been subject to an occasional strabismus, frequently resulting from long concentration of the eyes on one plane, as in reading. There was a loss of co-ordination both in the muscular movement of the globes and the power of adaptation to distance, and, in short, a want of balance in the tone of both organs. On looking into the mirror, as before explained, with an evenly adjusted amount of light on each eye, he observed the rays emerging from the left eye to cross and obliterate those from the right, which indicated to his own feelings and convictions, that the latter was the organ affected.

The advantage of such a test as the present to the ophthalmic surgeon must be obvious, when we consider that the sound eye has been frequently operated on in cases of strabismus, in consequence of incompetent diagnosis. *Medical Times, Sept.* 1, 1849, *p.* 176.

109.—*Treatment of Granular Ophthalmia.*—By M. Buys.—Some interesting, and at the same time decisive experiments were recently made at the Military Hospital of Bruges, to determine the relative values of nitrate of silver and acetate of lead in the treatment of the above-mentioned disease. They prove the incontestible superiority of the acetate of lead.

This remedy was first introduced into practice by M. Buys, of the Bruges Hospital. His mode of applying it is as follows:—The neutral acetate of lead is reduced to an impalpable powder, and applied to the eyelids by means of a very fine hair-pencil moistened with water, and then dipped in the powder. From one grain to one grain and a-half, is sufficient to cover the whole inner surface of one eyelid, over which it is uniformly spread. After two or three applications, the granules disappear, and several examples of a prompt cure are related by Dr. Cunier, in cases which the surgeons had acknowledged to be rebellious to nitrate of silver. The application of the lead is likewise much less painful.

Medical Times, July 28, 1849, *p.* 74.

110.—*On the Appearance of the Membrana Tympani in Health and Disease.*—By Dr. Kramer, Berlin.—By the most careful ocular inspection of well-nigh 4000 cases of ear-diseases, repeated on some of these many times, I have arrived at the most undoubting conviction that the *healthy* membrana tympani is entirely colour-

less, shining, and diaphanous, having a well-marked concavity externally; and that under the influence of the inflation of the tympanum, while the mouth and nostrils are closed, it is raised only into two round, longish swellings near the malleus, whence we may conclude that it is in a state of expansion. The *diseased* membrane, on the other hand, I have always found either inflamed, and then reddened, thickened, degenerated, swollen, or indurated; or, the inflammation having ceased, some of the numerous consequent changes of organization have been detectible, these likewise all excluding the idea of relaxation. With proofs so carefully obtained, I must demand of the supporters of the doctrine of the diseased tension and relaxation of this membrane to indicate some previous signs of the existence of such conditions, and for the future to abstain from all explanations derived from theoretical possibilities and pretexts. The membrane is visible in its whole extent, and changes in its tension must be detectible by a quick and practised eye; but since none such exist, we must omit them from among the really existing diseased conditions of the ear.

In the former edition of my work I put forth the opinion that the membrana tympani was never *ruptured* without prior inflammation. More precise observation has brought to my notice four cases in which the membrane has been ruptured by a blow on the ear, and therefore from the external compression of the air; and two in which this has followed the employment of the air-press, and consequently the pressure of air from within; but that the malleus should become broken within the tympanum of a living man, and, by the friction of the fragments, rupture the membrane (as stated by Ribes), I as little believe as that it also sometimes becomes ruptured by the pressure of accumulations of indurated cerumen in the meatus. In more than 400 of such accumulations I have never seen anything of the kind.

British and Foreign Medico-Chirurgical Review, Oct. 1849, *p.* 452.

III.—ON OPACITIES OF THE CORNEA.

By Dr. J. C. Hall.

[Dr. Hall remarks that it is very important to distinguish those opacities of the cornea which will be removed by the unaided efforts of nature, from those which will require the assistance of art. He says]

Opacities of the cornea, arising as they do from a great variety of causes, admit of division into three classes—

1st. Opacities resulting from the original structure having been destroyed by sloughing, or ulceration, followed by a deposit of opaque matter forming a cicatrix.

2nd. Opacities resulting from the application of a powerful escharotic.

3rd. Cases in which opaque matter is deposited upon the texture of the cornea, without any previous loss of structure.

Treatment of Opacities of the Cornea.—Simple nebula of the cornea can be removed without much trouble by the daily application of a solution of nitrate of silver in distilled water—(distilled water, seven drachms; nitrate of silver, one to three grains; wine of opium, without the aromatic, one drachm.) Or what is better still, in more severe cases, by the following drops, which I have now been in the habit of prescribing very frequently during the last ten years, and with much advantage:—Take of the bichloride of mercury, two grains; distilled water, an ounce: mix for a lotion, to be used once or twice a day. I would repeat, that whatever lotion be employed, it is far better not to drop it into the eye as we commonly see, even in hospitals; a camel-hair pencil should be dipped into the solution, and the opaque spot touched with it, by which means the remedy is at once brought into direct contact with the part to which you wish its operation to be confined, and that, too, without decomposition or dilutation from the tears, which must always be the case in the ordinary method.

Whoever has had much experience in diseases of the eye, will have observed that when any of these drops are employed for the purpose of removing opacities of the cornea, after a few weeks they appear to lose their useful effect, the disease becoming, as it were, invincible to them; I would, therefore, suggest that the nitrate of silver ointment. or drops, should be used for a week or ten days; then a weak solution of the bichloride of mercury; then the opium wine, and then the nitrate of silver a second time. This plan keeps up a continued state of improvement, and the disease is removed in a much shorter space of time than if one of them only had been persisted in.

When a patient comes before us with the worst possible form of opacity, leucoma arising, as is too often the case, from extensive ulceration, which has altogether changed the nature of the corneal surface, or with an extensive opacity, resulting from the application of a powerful escharotic, (which, although it may not have destroyed the vitality of the part, appears to have produced some chemical change in its structure,) it is quite labour in vain to attempt the cure of such an affection, and truly dishonest, time after time, to pick the pocket of the unfortunate patient of fees, by exciting hopes which can never be realized—hopes, the blighting of which, in the end, much increases his misery. Still, although this opacity cannot be destroyed by any means at our command, nor removed by the surgeon, this admission often only applies to the central portion of leucoma. In many cases we have the happiness to find the edges becoming gradually less and less opaque; a halo of hope surrounds this dimness of vision, and although it may not be within the compass of our art to improve the more dense central portion, something may yet be effected with the surrounding edges; and I have been consulted in cases where a steady continuance in the application of remedies has most certainly produced very great benefit, and the results recorded in the archives of the profession are doubtless such as to justify their use in many of these unfortunate cases.

According to my experience, the most useful remedy is the nitrate of silver ointment, of a strength proportioned to the condition of the affected eye. Counter-irritation should also be kept up behind the ears or at the nape of the neck by blisters. Many advise certain ointments or liniments for producing irritation on the skin. I have little practical experience of their effects, but a very short trial of them most fully convinced me that they were very inferior to blisters. In the first place, several days must elapse before any useful influence can be exerted, and, in addition to this loss of time, which is often highly important, I am much mistaken if this St. John Long practice of applying plasters of tartarized antimony, &c., above the eyebrows does not often produce permanently mischievous results, and too much care cannot be exercised in daily watching their effects, more particularly when applied to the head and face of young persons, in whom, in some cases, their application has been followed by a state of inflammation and sloughing which has even threatened the loss of life.

During certain stages of many inflammatory actions existing in the eye, setons and issues can be most advantageously employed. They are easily made by the surgeon, cause little pain or trouble, can be enlarged or diminished at pleasure, and being perfectly manageable, combine, within any limits we may be inclined to mark out, the advantage of a moderate degree of counter-irritation with a most salutary discharge. The advantages connected with the use of blisters are many—the convenience of their application—the rapidity of their operation—the quick subsidence of their effects when no longer required. There may, however, be cases in which, from peculiar irritability of the skin, from the liability of the patient to attacks of erysipelas, or from their having, on some former occasion, produced a violent effect on the urinary organs, some other form of counter-irritation must be used rather than blisters.

The following is the form in which I have usually employed the nitrate of silver ointment:—To take nitrate of silver, from three to ten grains; solution of diacetate of lead, twenty drops; lard, one drachm. This ointment must be used every night or every second night. A very small portion (not larger than a large shot corn) being put into the eye, it always creates more or less ophthalmia, and its application must be regulated accordingly.

During the employment of this ointment, I would in many cases most strongly advise the internal exhibition of the bichloride of mercury. When there is nothing to counter-indicate its use, I have given it for six or eight weeks in the following form, without any severe affection of the gums, irritation of the bowels, or any other symptom requiring that the dose should either be intermitted or reduced. Ordered solution of the bichloride of mercury, one drachm; tincture of bark, one drachm; distilled water, seven drachms and a half. To be taken twice a day.

As an alterative, the bichloride or oxymuriate of mercury, though doubtless more frequently prescribed than formerly in these cases, is not, I venture to think, so extensively used as it ought to be. It

can be given in solution, which is of considerable advantage, rendering its action much more certain, more equal, and by readier absorption probably more effectual in producing an alterative influence upon the whole system. Dr. Holland remarks that he has "seen its influence in augmenting the secretions, procuring the absorption of morbid growths, altering the state of the skin in many cutaneous disorders, and changing the character of morbid actions generally throughout the system, in cases where he believes no other medicine or combination of medicines would have had equal effect. Its combination with bark, steel, sarsaparilla, &c., afford resources of the greatest value in the treatment of disease; and, though otherwise held by common opinion, he thinks it, on the whole, as safe a medicine as calomel in the hands of the practitioner, inasmuch as its distribution can be made as equal and determinate, and its effects, from being given in a state of solution, are much less likely to be interrupted by mechanical hindrances in the stomach and bowels."

I would also add, and it is worthy of note, that this medicine (bichloride of mercury) may be continued in uninterrupted use for a very considerable period, without obvious injury or inconvenience, and in certain cerebral and spinal affections, a long unbroken course of this preparation is of singular avail, but to reap its fullest benefits, we must be watchful, patient, and decided in its use, for in cases in which, in the end, the happiest results follow the administration of the remedy, the changes are often the slowest, and not testified by those instant and obvious results which are sometimes desirable to fortify even the mind of the physician in the perseverance proper to the practice, still more to convince the patient, his family, and friends, that time is necessary for the development of the full and complete advantages of the means employed. Thus it frequently happens that the patient, alarmed, it may be, at the name of the medicine, and by the precautions taken as to its dose and effects, or tired by the little progress he appears to make, refuses to go on with the remedy, very often at the moment when becoming most effective, and when there was every reason for thinking a dangerous or distressing malady would eventually yield by a further employment of it. I have many times seen the beneficial results arising from a long-continued use of small doses of the bichloride of mercury, in solution, in chronic iritis, and also in several cases of paraplegia, the slow progress of the disease giving full scope for its effects, and the great danger in prospect, justifying a long trial of the remedy. Of course in strumous children, mercury in any form can seldom be employed with advantage, and in treating opacities of the cornea in them, in addition to appropriate remedies to the eye, it will be necessary to strengthen the system by attention to diet, shower-baths, sponging the body with cold water, the administration of the disulphate of quinine, and a residence of some months during the summer, when the circumstances of the parents will permit, at the sea-side.

Lancet, July 28, 1849, *p.* 85.

112.—ON A REMARKABLE SWELLING OF BOTH UPPER EYE-LIDS.

By William Bowman, Esq., F.R.S., &c.

[The following case is interesting on account of its novelty.]

Henry John B., æt. 16, a very healthy looking boy, of ruddy complexion, reddish hair, son of a publican at Islington, came to the Ophthalmic Hospital, Moorfields, in February 1848, for a swelling of both upper eyelids, which made his friends and himself uneasy by its unsightly appearance, though he had experienced no pain. The swelling was precisely alike on the two sides. It was almost limited to the outer half, or outer two-thirds of the lid, extending from the brow to within a sixteenth of an inch of the tarsal border, where it ceased by a groove, over which the relaxed and distended integument hung, as was very evident when the lid was viewed in profile. The swelling was reddish in the middle part, from fulness of the cutaneous vessels. It was quite soft, as if from œdema of the parts subjacent to the skin; and when pressed by the finger, no resistance was encountered, nor was any indentation left. The skin was thin, and slipped freely over the orbicularis muscle, nor was it easy to seize between the finger and thumb a fold of this muscular layer. When this was done, however, the feeling was that of redundant and loose cellular tissue beneath, and not of any tumour. Eversion of the lid showed the conjunctiva and subjacent tissue to be perfectly natural. Pressure behind the external angular process detected no tumour. The globe was in all respects healthy. Careful inquiry as to his general functions, only served to show that he was in excellent health.

He stated that this swelling had come on gradually, on both sides together, during the preceding three months, without pain or tenderness, and that he was first made aware of it by the remarks of his friends. On one morning it had been so much worse that he could not open the lids on waking; but this had subsided during the same day, and now, for some weeks, the affection had been such as I then saw it. It appeared to be a symmetrical partial œdema of the lids, of which the cause was concealed.

Not understanding the case, I first directed some purgatives, with alterative treatment, and an evaporating lotion to the lids, and subsequently gave him diuretics; but the conditions remaining the same, I ran some slender threads through the swelling on one side only, as an experiment, and left them in for a week, hoping there might follow some condensation of the tissues in their tracks; but when they were withdrawn, the swelling remained the same, but the integuments were rather redder. He now for three months, under the advice of my colleague, Dr. Farre, resumed the alterative remedies, with a strictly regulated diet, but without any good result.

During my absence from town in September, my friend, who was seeing my patients, gave him iodide of potassium, and applied

the tincture of iodine to the swollen lids; and the former I continued on my return, but left off the tincture, as it appeared to increase the tumefaction, by drawing more blood to the skin.

In November, he was just as in the preceding February. There was the same redness and bulging of the skin, and the same feel of the parts; neither tenderness nor itching. His health was good. I noticed that his temporal fossæ were unusually large, but, as it afterwards proved, this had no bearing on the case. I had also a drawing made; but a wood-cut would not convey the appearance, as the redness of the integument was even more conspicuous than its swelling.

The attempts hitherto made failing to remove the swelling, they were altogether laid aside, and I waited till the spring, to see whether by that time any spontaneous improvement might not take place. At the end of April, the young man again presented himself, and being exactly in the same state as before, he was anxious that I should perform whatever operation offered a chance of improving his appearance. I accordingly determined to employ a modification of the operation for entropion, and to remove, not merely a horizontal ellipse of integument from the most projecting part of the swelling, but also a corresponding portion of the orbicularis muscle and of the fascia below it, and so to endeavour to consolidate the integuments with the parts beneath, which seemed the principal seat of the disease.

On the fourth of May I operated on the left side. A piece of the integument was taken up with the entropion forceps, and removed with scissors to the extent of two-thirds of the horizontal width of the lid, and one-third its vertical depth. The orbicularis thus exposed was healthy, and was removed to nearly the same extent, by means of common forceps and scissors; a dense cellular fascia then bulged forward in the gap. This fascia being removed in its turn, a mass of fat, resembling the natural fat of the orbit, and about as large as an almond, fell forward in the opening, and I immediately removed it. It was not tightly embraced by any capsule of the areolar tissue which surrounded it, but was divided into pellets, or small lobes, which moved freely on one another. It therefore had not the appearance of a fatty tumour. After its abstraction, there was no other tissue projecting, and I therefore closed the wound by sutures, and had the satisfaction of seeing it heal in four days, with an almost complete relief of the deformity.

A fortnight after, at his own request, I performed precisely the same operation on the right side, and with the same result. On cutting into the sub-muscular fascia, a pellet of fat appeared, and was cut away; a lobe of what seemed to be lachrymal gland was then exposed to view, but, showing no disposition to project, it was not interfered with. When the wound healed, the deformity was even more completely removed than on the other side, more care having been taken with the shape of the piece of integument excised Two months afterwards, the lids hardly bore any trace of what had occurred.

Remarks.—Probably most surgeons of large experience in eye diseases, have occasionally met with a case like the preceding, in which a swelling, resembling œdema, and of which the most obvious character was a distended and somewhat pendulous and reddened condition of the skin of the upper lid, occurred simultaneously on both sides, in a young person otherwise in perfect health. I have myself seen but two others besides this one, and in neither of these was the swelling known to depend on the protrusion of fat, although I have little doubt that such was actually the fact. In both, the upper lid on each side was similarly affected, and both were young subjects.

The true fatty tumour of the orbital cellular membrane is comparatively rare, and it occurs singly, generally at the upper or outer side of the globe, in the loose areolar tissue outside the recti muscles. When sufficiently large to cause a prominence of the skin, it admits of being felt as a tumour, although readily retiring into the orbit under pressure; and on everting the lid, it usually projects under the conjunctiva, where that membrane is reflected from the lid to the globe, near the outer canthus.

London Journal of Medicine, Nov. 1849, *p.* 989.

113.—*On the Use of Glycerine in Deafness.*—By T. H. Wakley Esq., Surgeon to the Royal Free Hospital.—[Mr. Wakley states that after employing the cotton-wool as recommended by Mr. Yearsley (see Retrospect, vol. xviii, p. 295), in a great number of cases of deafness, and finding that the relief was frequently only temporary, he was led to the conclusion that the good effects were due to the water with which the cotton was moistened. He was therefore induced to employ glycerine, as being a substance which would retain its moisture. Mr. W. states that he has used it in several hundred cases, in many of which the power of hearing has been completely restored. He says]

It was only after much experience in the application of the glycerine, and from observing its action in a great number of cases, that it could be ascertained what were those conditions of the ear in which it was most likely to prove of advantage. Contrary to what might have been anticipated, the use of the remedy was successful in persons in whom the deafness had been of many years' duration—one, for example, thirty years, and also in cases where the existence of the malady could be traced to the eruptive fevers of childhood. In instances of deafness caused by inflammation, followed first by suppuration, and then by a horny, dry condition of the auditory canal, the application of glycerine has been attended with signal advantage. Equally marked and peculiar is the success when it is used in cases where there is a partial or total absence of ceruminous secretion. In many instances of deafness belonging to these classes of cases, the employment of glycerine has been followed by a perfect restoration of the power of hearing. In other examples of deafness, where the membrana tympani had evidently become thickened and hardened, and on

examination with the speculum, denoted a whitish or pearly appearance, the use of the glycerine was followed by strikingly beneficial and gratifying effects. It is evident, therefore, that the application of glycerine is equally admissible, whether the tympanum be in a sound state, or whether it has been destroyed by ulceration.

Lancet, June 16, 1849, *p.* 631.

[Mr. Wakley makes the following additional remarks on the mode of using the glycerine. He begins by referring to a former remark, that:]

"The ears were carefully cleaned by means of cotton held between the blades of a pair of forceps, and dipped frequently in warm water. The canals were then rubbed with dry cotton, held in a like manner. Then the glycerine was applied by the same means, the cotton, well soaked in it, having been repeatedly passed backwards and forwards in each external meatus, care having been taken to apply it to the tympanum." It was mentioned, in some of the other reports, that the glycerine had been applied by means of a camel-hair brush. In addition to the two methods now noticed, I ought to have stated, that an instrument has been used by me, which somewhat resembles the caustic-holder employed in diseases of the uterus. The length of the instrument is six inches. The shaft about its middle is divided into three parts, which, at their extremities, form as many claws. These claws can be made to grasp the cotton firmly, by means of a sliding ring. The cotton, on being placed between the claws, should be so fixed that in moving the instrument to and fro in the meatus externus, the free passage of air should be provided for, otherwise an imperforate tympanum might be ruptured. The two best modes to be adopted for accomplishing this object are—1st, by not placing too large a quantity of cotton within the claws; and 2ndly, by so fixing the cotton, as to allow portions of it slightly to project in fringed ends, in the three divisions formed by the claws. The extreme points of the claws should *dip* or *turn* towards the shaft. The tympanum will not then be scratched by them should the cotton, from having been carelessly fixed, accidentally escape.

A further experience of the effects of glycerine, since the case reported in March was written, has induced me to believe that I have claimed much less credit for that agent than its merits deserve. In several cases of deafness of very long standing, and in which the aural passage and tympanum exhibited a white, polished appearance, and the external meatus had become inelastic, hard, and horny, and entirely deprived of its natural functions, the *glycerine*, patiently applied, daily, has proved of the utmost advantage, by restoring the parts to their natural condition, and by reproducing the ceruminous secretion of the organs.

Lancet, June 23, 1849, *p.* 664

[Mr. T. Buchanan, of Hull, claims the merit of having introduced the use of glycerine in diseases of the ears.]

Medical Times, June 30, 1849, *p.* 684.

MIDWIFERY,

AND

THE DISEASES OF WOMEN.

114.—ON THE POSITION OF THE FŒTUS IN UTERO, AND ON THE CAUSES OF PRETERNATURAL PRESENTATIONS.

By Dr. J. Y. Simpson, Edinburgh.

[Dr. Simpson endeavours to establish the following propositions relative to the common or natural position of the fœtus in utero:]

1. The usual position of the fœtus, with the head lowest, and presenting over the os uteri, is not assumed till about the sixth month of intra-uterine life, and becomes more frequent and more certain from that time onwards to the full term of utero-gestation.

2. Both the assumption and maintenance of this position, are vital and not physical acts, for they are found to be dependent on the existence and continuance of vitality in the child; concurring with its life, but being lost by its death.

3. In human physiology we do not know or recognize any vital power or action, except muscular action, capable of producing motions calculated to alter or regulate the position, either of the whole body, or of any of its parts; and further, the motory muscular actions of the fœtus are not spontaneous or voluntary, but reflex or excito-motory in their nature, causation, and effects.

4. The position of the fœtus, with the head placed over the os uteri, is that position in which the physical shape of the normal and fully developed fœtus is best adapted to the physical shape of the normal and fully developed cavity of the uterus.

5. This adaptive position of the contained body to the containing cavity is the aggregate result of reflex or excito-motory movements on the part of the fœtus, by which it keeps its cutaneous surface withdrawn as far as possible from the causes of irritation that may act upon it as excitants, or that happen to restrain its freedom of position or motion.

[Proceeding to speak of preternatural positions and presentations of the fœtus, as originating in the derangement of one or other of these normal conditions, Dr. Simpson states them to be referrible to the following causes:]

First. Prematurity of the labour; parturition occurring before the natural position of the fœtus is established.

Secondly. Death of the child in utero; or in other words, the loss of the adaptive vital reflex actions of the fœtus.

Thirdly. Causes altering the normal shape of the fœtus or contained body, or causes altering the normal shape of the uterus or containing body, and thus forcing the fœtus to assume, in its reflex movements, an unusual position in order to adapt itself to the unusual circumstances in which it happens to be placed.

And lastly. Preternatural presentations are occasionally the result of causes physically displacing either the whole fœtus or its presenting part, during the latter periods of utero-gestation, or at the commencement of labour.

[*Prematurity of the labour* is a cause of mal-presentation in this restricted sense, that the position of the fœtus with the head lowest, and presenting over the os uteri, is not generally assumed till the sixth month; and that from this period such position is taken and kept with a certainty and frequency which increase in proportion to the duration of the pregnancy. Thus, Dr. Simpson states, that while the per-centage of head presentations in children born at the end of the sixth month is only about 52 in 100, it increases during the seventh month to 68 in 100, and to 76 in 100 during the eighth and ninth months; and rises at the full period, as high as 96 or 97 in 100. He says:]

It is hence evident that if, from any causes, parturition happens to come on prematurely, the child is much more liable to present preternaturally, than if pregnancy had gone on to the full time; or, in other words, the prematurity of the labour is, in this sense, a cause of the malpresentation of the fœtus.

[From an examination of statistics published by various authors, Dr. Simpson educes also the following conclusions:]

1. That while presentations of the head occur in 96 per cent. in common obstetric practice at the full time, the same presentations occur only in 70 per cent. among premature labours; 2. That pelvic presentations are nearly eight times more frequent among premature labours than among labours at the full time; and 3. That transverse presentations are nearly ten times more frequent among premature labours than among labours at the end of the usual time of pregnancy.

[On the subject of the *death of the child*, as a cause of abnormal presentation, Dr. Simpson says:]

The child not unfrequently dies in utero, and before labour begins. In cases in which the death of the fœtus is induced from any cause, during its intra-uterine life, the child, when labour at last supervenes, is apt to be found presenting preternaturally. The maintenance as well as the assumption of the usual position of the fœtus with the head downwards and over the os uteri, is an excito-motory, and consequently a vital act; and hence, when the vitality of the

fœtus is lost, its position, as a result of that vitality, is liable to be lost also. In other words, the death of the child thus becomes a cause of its malpresentation at the time of birth.

The difference between the liability to cephalic, pelvic, and transverse presentations between children who have died before labour, and those born alive, may be expressed as follows:- 1. Head presentations are 16 per cent. less frequent among dead than among living infants; 2. Pelvic presentations are five times more frequent among dead than among living children; and 3. Transverse presentations are four times more frequent among the former than the latter.

Monthly Journal, July, 1849, *p.* 866.

115.—*On the Rationale of Parturition.*—By Dr. Marshall Hall. [Dr. Hall recommends an experimental investigation into the rationale of parturition. He says]

For my own part, I *compare* parturition with the act of deglutition, and with the act of vomiting, and especially I regard it as combining.

1. *Reflex action;* 2. *Peristaltic action;* 3. *Voluntary*, and 4. *Emotional influence*, with 5. *Sphincteric relaxation.*

The question is—how far the first two sources of action can be severed, and measured, as it were, in experiment.

For this latter purpose, an instrument, to be designated a dynamometer, and consisting of a long tube attached to a small sphere of India rubber, and partly filled with mercury, may be employed. By means of this instrument we may reduce the reflex and peristaltic powers, in some measure, to numbers.

But first let me premise, that I propose hereafter to designate the reflex action and acts by the term *diastaltic.* The functions of all the *tubal orifices* and *exits* of the animal frame will be thus distinguished from all that is interior, as *dia*-staltic, the latter being *peri*-staltic, a term with which we are all perfectly familiar.

These words are very expressive and well contrasted. Others may be added, if this be deemed necessary, from the same root. An incident nerve may be designated *ana*-staltic; a reflex nerve, *cata*-staltic, &c.

The uterine actions are partly, I should think chiefly, diastaltic and partly peristaltic; and so of the other acts of ingestion and of expulsion, which are also still further influenced by emotion, modified by volition, and conjoined with sphincteric relaxation.

I formerly performed some interesting experiments on the rabbit and the turtle; which will be found in my volume "On the Diseases and Derangements of the Nervous System," §§ 323, 330. The œsophagus in the former, and the cloaca in the latter, are both diastaltic and peristaltic. The parturient uterus is doubtless so too. Its action may, we know, be excited through the spinal marrow, even from the mamma; and if detached altogether, and placed on

the table, I have no doubt that, like the isolated œsophagus of the rabbit, it would slowly expel its contents. Dr. Tyler Smith actually observed this event, some months ago, in a small bitch which had died without delivery. The abdomen and uterus being opened, the puppies escaped, and the uterus collapsed, or rather, contracted, so as almost to disappear.

I need scarcely refer to the observations and experiments of M. Brachet, and to the facts on record of post-mortem parturition in the human subject.

In regard to new experiments, this is what I would beg to propose:—

Having placed the animal under the influence of chloroform, and having introduced the dynamometer, first within the abdomen, and afterwards within the uterus, let us gently irritate.

1. *The Mamma;* 2. *The Vagina;* 3. *The Ovarium;* 4. *The Interior of the Uterus*, and apply a douche of cold water to 5. *The General Surface.*

Let us then observe the effect of irritation of the spinal marrow, as we proceed to its division or removal, upon the same instrument, similarly placed.

Let us then remove the uterus entirely, place it on the table, introduce the dynamometer, and observe the effect of

1. *Peristaltic action;* 2. *Various irritations;* 3. *Galvanism, &c. &c.*

Lancet, Oct. 20, 1849, *p* 438.

116.—*On the Movements of the Uterus.*—By Dr. W. Tyler Smith.—All muscles contract when subjected to immediate irritation, after they have been cut off from the influence of the cerebral and spinal centres, and when they have been as far as possible deprived of their nerves. In the case of muscles supplied by cerebral or spinal nerves, the contraction is limited to the spot irritated, and ceases with the removal of the irritation; but, in organs partly or wholly supplied by ganglionic nerves, as the heart, bladder, intestines, œsophagus, &c., the motion produced is of a peristaltic kind, spreading, generally in a vermicular manner, to a distance from the point of irritation, and continuing for some time after the irritation has ceased. The uterus is eminently endowed with this form of contraction. When one point of the uterus is stimulated through the abdominal parietes, the contraction excited extends with the utmost rapidity to the whole organ; the same occurs when the fingers are made to irritate any point of the internal surface of the uterus. Harvey beautifully described this peristaltic form of action in the uterus of the doe; William Hunter saw it in the cat and rabbit; Professor Müller in the uterus of the rat and the oviduct of the turtle; I have seen the same thing in the uterus of the guinea-pig and other animals. *This form of motor action is the basis of the other uterine actions.* In natural labour in the human female, it is, so to say, disguised by the reflex actions; but, under certain circumstances, it is able to effect, unaided, the expulsion of the child.

British and Foreign Medico-Chirurgical Review, July 1849, *p.* 10.

117.—*On the Impropriety of Frequent Operative Interference in Midwifery.*—By Dr. R. Collins, Dublin.—[Dr. Collins gives an account of 3847 labours attended by the late Dr. Joseph Clarke, of Dublin, in which he used instruments remarkably seldom. Dr. Collins says]

It may be observed he only used the forceps once, and that without completing the delivery. If we seriously reflect upon the happy results to the *mothers*, from the practice pursued by this distinguished physician, as regards the use of instruments, and then carefully examine the succeeding section upon children *still-born*, and find here equally happy results (as of the 3,816 *single* births, there was only *forty-two* children still-born of those that had arrived at the full period of gestation, or in the singularly small proportion of one in 91), we cannot fail to discover a number of *astounding* truths, sufficient to warn our *artificial* advocates and make them pause until *they can supply* their professional brethren with a series of facts equally satisfactory.

How seldom should most practitioners be found to use instruments, if the successful course pursued by Doctor Clarke were universally aimed at. Is it not worthy of our best consideration, with the invaluable statement before us, that, in an extent of practice in the upper ranks of life perhaps unexampled, there is *not one single instance of death* resulting from *laborious* or *protracted labour!* This is a practical fact, which ought to be carefully recollected, and seriously weighed, by most of our continental brethren, who use instruments in every 5th, 10th, 15th, 20th, or 30th labour under their care, with the object of expediting delivery; as also by some of our own countrymen, whose unsound doctrines inculcating mischievous interference to promote *hasty* delivery, the unquestionable truths here recorded clearly demonstrate to be most unjustifiable and most uncalled for. Should not this inexpressibly important record for ever silence those who venture to publish crude and fanciful opinions, unsupported by any data from their own experience affording similarly happy results. It affords me infinite satisfaction to supply this truthful registry of facts for the universal and serious consideration of the profession. These truths speak in language the most convincing, and must, when studied, leave an indelible impression.

Medical Gazette, July 20, 1849, *p* 122

118.—*Case of Post Partum Inertia Uteri, unattended by Hemorrhage.*—By Dr. C. H Higgins.—[Notwithstanding that in ordinary cases, the only safeguard against post-partum hemorrhage is the equable and regular contraction of the uterus, yet the following case proves that the complete absence of such contraction for a considerable time after delivery, is not necessarily attended with hemorrhage:]

On the 8th July 1843, I was summoned to a Mrs. H., æt. twenty-five, a person of leucophlegmatic temperament, who had been in labour for some hours with her first child. She was delivered of a

live female infant at nine p.m., by means of the short forceps, the powers of the uterus failing after the head had descended to, and was resting on, the perineum; on which account, also, after a considerable interval, the placenta required to be extracted. The separation of the secundines was easily accomplished, but independent of any expressive efforts on the part of the uterus, which remained passive, and was perceived through the abdominal walls to be quite placid. Suspecting that clots must be distending its cavity, and much alarmed with the apprehension of flooding, I immediately introduced the hand into the womb for the purpose of removing these, but was surprised to find the organ already quite empty, and almost fully dilated, and showing no disposition to contract. Hoping to excite its action, I retained my hand in the cavity for nearly the space of two hours, keeping up, at the same time, firm grasping pressure through the abdominal parietes, with the other hand; but all in vain, the organ remaining entirely passive. At length, perceiving not the slightest tendency to hemorrhage, and worn out by anxiety, long watching, constrained position, and want of refreshment, I ventured to remove my hands, and putting on a firm abdominal bandage with a pad over the uterus, I left the patient, with whom I had now been uninterruptedly fourteen hours. In the course of half an hour I revisited her; no bleeding had manifested itself, and the uterus still remained patent to a great degree, although between two and three hours had elapsed since the separation of the placenta. Contractions, however, were gradually taking place, and the uterus ere long assumed the size and condition usual after delivery.

The labour in this case was remarkable for nothing but its lingering and atonic character. The patient's strength or courage never flagged, and she made a good recovery without a single drawback. She has since borne two fine children, her labours being characterised only by the same want of tone as in the first instance.

Monthly Journal, July, 1849, *p.* 898

119.—CASE OF POST-PARTUM HEMORRHAGE TREATED BY COLD INJECTION OF THE UTERUS.

By Dr. W. Tyler Smith.

[This was a case in which the placenta adhered, but it had been removed artificially before Dr. Smith's arrival. Hemorrhage continuing, notwithstanding the application of a tight bandage to the abdomen and cold to the vulva, ergot and brandy were given, but with no beneficial effect.]

Matters becoming urgent, the abdominal bandage was removed, and the abdomen being laid quite bare, cold water was poured upon it from an ewer, held at a considerable height. After cold douching in this manner, warm flannels, with frictions to the abdominal

surface, were used, in order to obtain the full stimulus of *alternation*. But even these energetic measures failed in producing any decided contraction of the uterus or the arrest of the flooding.

Such being the state of the case, and feeling great anxiety for the patient's life, I now very cautiously introduced my hand into the uterus. The ostium vaginæ was contracted, but the vagina itself was very large, relaxed, and full of coagula. This great distention of the vagina, and the extreme relaxation of the abdominal muscles, appeared greatly to favour the relaxation of the uterus, and consequently the hemorrhage. The uterus was as soft and uncontractile as a pouch of wash leather.

I kept my flattened hand in the inert uterus, irritating, at the same time, in the most gentle manner, the internal surface of the organ with the tips of the fingers. Great and sustained pressure was made upon the abdomen externally by Dr. Fairbrother, and after a while, when his arm and hand became numbed by exertion, an assistant supported his elbow, so as to keep up the pressure. In this manner, uniform and sustained pressure was applied to the abdomen over the uncontracted uterus. The abdominal walls were too lax and prominent to admit of pressure upon the abdominal vessels above the uterus. By the effect of the direct uterine pressure, the bleeding organ was compressed very completely, and the hemorrhage became more under command; still the uterus itself did not contract, and an oozing went on from time to time, now that we felt every ounce of blood to be literally of vital consequence. It was evident that the hand could not be withdrawn, or the external pressure remitted, without the utmost hazard of death.

After a temporary recovery from fainting, we made our patient swallow lumps of ice at short intervals. As each lump passed into the stomach, there was a distinct but transient contraction of the uterus. The organ would quiver upon my hand for a second or two, and then become inert, as before. There was no safe or continued contraction to be produced by these means. The pulse continued rapid and fluttering, the breathing gasping and hurried, the eyes glassy, prominent, and fixed,—every moment, in fact, seemed to threaten dissolution. On two or three occasions, while we were thus engaged, there were sickness, syncope, and a sudden giving way of all the sphincters, just as in an animal in the very act of dying,—the skin being cold, relaxed, and covered with perspiration.

It would be, indeed, invaluable, if we could diagnosticate, in patients reduced to this extremity, between those cases in which the syncope is attended by uterine contraction, and in this way contributes to the arrest of the hemorrhage, and those other cases in which syncope is the commencement of the series of sinkings which end in death—syncopal dissolution. I believe there is no other diagnosis except the contraction or non-contraction of the uterus. The syncope with contraction is favourable in its results; the syncope without contraction is most dangerous, and often mortal.

As a last resource, I now had a quantity of iced water injected into the cavity of the uterus, in the following manner:—The pipe

of a common enema-syringe was passed a little way into the vagina, between my arm and the perinæum, and the iced fluid was then suddenly injected along the side of the arm, until it filled the whole of the vagina, and penetrated into the cavity of the uterus. As the cold water ran through my fingers, converting the whole hand into a cold tampon, I had the great happiness of feeling the uterus contract decidedly for the first time, so as to assume its rounded form. The uterus now stood up, as it were, to its work; and we were able, at this point, to relax the external pressure somewhat. I could now close my hand in the uterus, and the internal surface, from being spongy and shapeless, became smooth, elastic, and rounded.

After a short time, there being slight signs of reaction, the pulse, which had been carefully watched by Mr. Sewell, sinking to 100, and no more hemorrhage occurring, we consulted together, and I afterwards slowly withdrew my hand out of the uterus, placing, as a precautionary measure, a large piece of ice in the vagina. No hemorrhage followed the withdrawal of the hand, nor did any occur after this time. We were all considerably exhausted by upwards of two hours of excessive anxiety and no little exertion; but the life of our patient was happily saved.

One grain of opium and three grains of calomel were prescribed. The patient took a little arrowroot, and was kept, until after twelve o'clock, p.m., in the position she had been placed in during the flooding. Dry clothing was put about her, and the night was passed satisfactorily, and with scarcely a stain of discharge.

The after-treatment was of the most simple kind. The following day an injection of warm water into the vagina was recommended as a soothing measure. Slight abdominal tenderness arose, but was removed by a repetition of the opiate, and a sinapism to the abdominal surface. As nearly as we could ascertain, about four or five pints of blood were lost. For a week or two there were the usual symptoms of loss of blood; but the lochia appeared naturally, the milk was secreted in good quantity, and the patient was able to suckle her infant.

Remarks.—In this case, treated physiologically, all the motor powers of the uterus were attempted to be called out, and in vain, until the organ had recovered its nervi-motor power. Exhausted by delivery, the uterine energy was quite paralyzed, and for a time the organ refused to obey every form of stimulus.

The *direct* spinal actions of the uterus were stimulated by the ergot of rye, by the brandy, and by the maternal emotions. These remedies were directed from the spinal centre straight to the uterus, and with but little effect.

The purely *reflex* actions of the uterus were attempted to be excited by the cold douching of the vulva, by the sudden cold, alternated with frictions and warmth to the abdominal surface, and by the ice taken into the stomach.

The *peristaltic* uterine action was stimulated as far as possible

by the immediate irritation of the uterus, by internal and external pressure, and by cautious digitation upon the internal surface.

The influence of the iced injection was partly to excite *reflex* action by impression upon the internal uterine surface, and partly to stimulate the uterine muscular fibre itself. This latter effect it would have had if the uterus had been wholly separate from its nervous supply, in virtue of its muscular *irritability*, whilst the former action depended upon the uterine excito-motor nerves, and the spinal centre.

The flow of blood was attempted to be arrested *mechanically*, in the first instance by the abdominal pad and bandage, and afterwards more successfully by the hand in the uterus acting as a plug, and as an auxiliary to external pressure, In the nervi-motor condition of the uterus in this case, nothing could have compensated for the mechanical arrest of the flooding for a time.

Side by side with these energetic remedies, the strength was supported by brandy; the respiration was kept up by sprinkling the face with cold water, by fanning, and by open doors and windows; the effect of loss of blood upon the cerebrum was diminished, as far as possible, by position; and everything was done by word and deed to keep alive the hope and confidence of the patient, while she remained sensible.

Though the injection of the iced water had the chief merit of inducing the final contraction of the uterus, I do not think anything else could have been properly omitted, and I question if this application would have been equally effective at an earlier period of the case; nor should we have been justified in resorting to it, or, indeed, in introducing the hand into the uterus, until other means had failed The uterus was so thoroughly dead to all stimuli, that nothing but the mechanical arrest of the flooding for a time sufficiently long to allow the uterus to recover its excitability, would have been sufficient. To gain time in such cases, without further loss of blood, is to save life. Throughout, I was most ably seconded by Dr. Fairbrother and Mr. Sewell.

With respect to the introduction of cold or iced water into the uterus. Of its excessive value in the present case there can be no doubt whatever. Injection of the uterus, in numerous forms, is a subject which has attracted much attention of late. Some have recommended its injection simply by way of the vagina, but unless the tube be placed within the os uteri, and *retained* there, we cannot be certain of injecting the uterine cavity. Others have recommended various mechanisms for placing a bladder in the uterus, and injecting this artificial reservoir. But it is a difficult matter, in a case of hemorrhage, to place a bladder in the uterus so that it shall be retained there. And why use a bladder at all? The contact of cold water with the surface of the uterus is of no more consequence than the contact of the cold bladder. It has indeed been *said* that the distended bladder will exert a pressure on the internal uterine surface, sufficient to arrest the bleeding mechanically.

But, in order to believe this, we must forget the obvious truth, that the more the uterus distends, unless (impossible paradox) it should contract as it distends, the more profuse would be the flooding. No! there is but one real principle in all the forms of apparatus which have been proposed and engraved—namely, the excitement of uterine contraction by the direct application of cold to the lining of the uterus. I believe, therefore, that such inventions, however ingenious, are, with all respect for their authors, works of supererogation. If the hemorrhage be so excessive, and the uterus so dead, as to resist all ordinary stimuli, the injection of iced water with the hand introduced into the uterus, will I think, be found a more straightforward and efficacious plan than any of the uterine bladders or injections which have been proposed. With the hand in the uterus, we are informed when the uterus is really injected, and we learn the effects of the injection. The hand is both a *guide* to the operation and a *record* of its effects.

There is scarcely a more important point in the treatment of uterine hemorrhage than the introduction of the hand into the uterus. While I would deprecate it in ordinary cases, and until other measures had been tried in vain, it is undoubtedly of extreme value in such cases as that which I have related, when the remote danger of uterine inflammation fades into insignificance, in the presence of the imminent and instant danger which already exists. But when introduced, it should be with a view to aid the effects of external pressure, and to arrest the hemorrhage mechanically, rather than with the idea of stimulating the uterus to immediate action. By this means we may assist the mechanical arrest of the flooding, and gain time for the recovery of uterine power. We may press vigorously upon the uterine walls, from without; but unless the hand be in the uterus, to measure the amount of the pressure, and its effects, and to form, as it were, an internal pad, upon which the pressure may be exerted, the patient may die while the abdomen is being compressed with considerable force.

Lancet, July 7, 1849, *p.* 6.

120.—*On the Pathology of Puerperal Convulsions.*—By Drs. Cormack and Tyler Smith.—[At a meeting of the Westminster Medical Society, Dr. Cormack detailed several cases with a view to point out the connexion between renal congestion and puerperal convulsions.]

He considered puerperal convulsions to be—though not always, yet generally—the toxicological results of non-elimination of the excretion of the blood; and that, in by far the greater number of cases, this non-elimination depends on renal congestion, caused by the pressure of the gravid uterus. Œdema and albuminuria are frequent concomitants or precursors of convulsions, as shown by Dr. Lever and by MM. Devilliers and Regnault. The gravid uterus, or any tumour pressing on the renal veins, must cause congestion of the kidneys, and consequent toxæmia; and this is the more injurious

to the pregnant woman, as her blood requires an extra degree of depuration, both from excrementitious matter from the fœtus, and also from the elements of milk. Retention of these should, Dr. Cormack thought, be considered as the cause not only of convulsions, but also of various other distressing symptoms occurring during pregnancy. Uterine epilepsy probably often arises from toxæmia; and the suppression of the lochia may induce post-partum puerperal convulsions. When convulsions recur after delivery, we must suspect structural renal disease. The explanation of delivery generally arresting convulsions is not so much that uterine irritation is lessened, as that the hyperæmic state of the kidneys is relieved. The most common subjects of puerperal convulsions are strong, healthy, young women, pregnant for the first time; and an examination of the cases recorded by authors proves this fact. In them the abdominal walls are most unyielding, and unable to relax under the pressure of the gravid womb. Cases of puerperal convulsions in subsequent pregnancies might be either toxæmic or non-toxæmic, the toxæmic cases might be classed under the following heads:—1. Persons who had never gone to the full time. 2. Persons of extreme muscular development. 3. Persons suffering from structural disease or obstruction of the kidney. 4. Excessive volume of uterine contents, including twin cases, &c. Dr. Cormack was desirous of drawing attention to toxæmia as a cause of puerperal convulsions, and also of recognising non-toxæmic convulsions. He thought that Dr. Tyler Smith, who had treated this subject more philosophically than any preceding writer, had, while recognising toxæmia, attached too little importance to it. The following diagram shows the relation of toxæmia to puerperal convulsions:—

PREGNANCY
causes

- Increased necessity for depuration of blood.
- A tumour (gravid uterus) causing renal congestion, which causes
  - Non-elimination of poisonous excrement from blood, TOXÆMIA, which causes
    - Action on brain, spinal marrow, and medulla oblongata, hence result CONVULSIONS.

Dr. Tyler Smith expressed his sense of the importance of the view taken by Dr. Cormack. He considered it would be difficult to estimate too highly the influence of impurity of the blood, as a direct irritant of the nervous centres in pregnancy. It was necessary to take a comprehensive view of the causes of blood-poisoning

dependent on the pressure of the gravid uterus. There was the pressure on the intestinal canal, causing constipation; there was the pressure upon the emulgent veins, causing albuminuria, and the retention of urea in the blood; there was the pressure upon the hepatic vessels, which he had frequently observed to produce pink deposits in the urine; and lastly, there was deficient oxygenation of the blood from pressure upon the thoracic viscera. All these agencies interfered with the proper depuration of the blood in advanced pregnancy, and often produced distressing nervous symptoms. But it was curious to observe that during pregnancy certain vicarious or complementary secretions and excretions were set up, which tended to preserve the blood in a healthy state. There was the sickness and vomiting of pregnancy, the salivation which sometimes occurred, the increased action of the glands of the axillæ and of the skin generally, and the secretion of milk by the mammæ, which was sometimes profuse during the latter months, particularly in cases of albuminuria. All these actions tended to compensate, and were no doubt intended to compensate, for the effects of pressure upon other organs. The fœtus also must be considered as an excretion, so far as the mother is concerned; and a large quantity of matter, which would otherwise have to be eliminated as effete from the maternal blood, went to form the liquor amnii, the membranes, the bones of the fœtus, the meconium, fœtal urine, &c. These points required consideration in our estimate of the state of the blood in gestation. Still, there could be no doubt that in many cases an impure state of this fluid, a true toxæmia, did obtain, and affected the spinal centre, the organ of convulsions, in a centric or direct manner. He believed this to be a predisposing rather than an exciting cause. It was only when the toxæmia was very intense, as in poisoning by carbonic acid, that convulsions, depending solely upon the state of the blood, occurred. If it were otherwise, convulsions would often occur independently of the excitement of parturition. But we generally saw that convulsion was actually excited when the predisposition existed, by some manifest irritation acting in a reflex manner, such as the irritation of the parturient passages during labour, or irritation of the stomach, bladder, or intestines.

Medical Gazette, June 15, 1849, *p.* 1046.

121.—*On the Phenomena of Generation.*—By PROFESSOR OWEN. —The essential conditions appear to be a nucleated cell, the product of a nucleated cell, and the combination of the two; the ovum having received the matter of the spermatozoon, is said to be impregnated, and the phenomena that thence arise are essentially the same, up to a certain point, in all animals, and consist of the formation of a germ-cell and its propagation of a numerous offspring, at the cost of the germ-yelk, by a series of reiterated spontaneous divisions. The progeny of the primary impregnated germ-cell, which may be called secondary germ-cells, constitute the germ-mass; this progeny resemble their parent with a diminution

of size, to a certain stage of descent, when they may be ultimately reduced to their essential parts or nuclei. But when they cease to exist as germ cells, they minister to the life of a being higher than themselves, combining to construct its tissues, and to impart important properties to its fluids. But, says Professor Owen, not all the progeny of the primary impregnated germ-cell are required for the formation of the body in all animals; certain of the derivative germ-cells may remain unchanged and become included in that body, which has been composed of their metamorphosed and diversely combined or confluent brethren; so included, any derivative germ-cell, or the nucleus of such, may commence and repeat the same process of growth by imbibition, and of propagation by spontaneous fission, as those to which itself owed its origin; followed by metamorphoses and combinations of the germ-masses so produced, which concur to the development of another individual.

London Journal of Medicine, July, 1849, *p.* 629.

122.—*Note on the Physiology of Impregnation.*—By Dr. H. Oldham. [A case came under Dr. Oldham's notice, in which he states, he was well assured,]

That the last day of her last menstrual period was the 16th of January—that sexual intercourse occurred on the 28th of January—that it had not taken place for several weeks before, and has not been repeated since, and consequently she was impregnated twelve days after menstruating. This has an important experimental reference to the modern theory of generation, which makes the menstrual period correspond with œstruation in the lower animals, and limits the period of conception to a day or two before, and about eight days after the flux. During this time it is affirmed that an ovum has been spontaneously cast off from the ovary, and during its slow movement through the sexual passages, that it may be impregnated; but should it not be impregnated, that the female cannot conceive until a day or two before the next menstrual or œstrual time. The physical impossibility of pregnancy during this interval is not blinked by the supporters of this theory. You see if this were true it would have a most important and practical bearing. It constantly happens that cases come before us where either from disease of the uterus or pelvis, or sexual organs, or exhaustion from frequent abortions or protracted labours,—that it would be most desirable to suspend for a time or altogether prevent pregnancy. And this might be done were this really a physiological law, and without imposing a much greater restraint on sexual indulgence than does the old Rabbinical law. But the truth is, that this theory has been prematurely shaped into a law, and it will not, I am sure, bear a practical test. I know of cases, which I have carefully inquired into, where impregnation occurred at the respective times of ten days, twelve days, and twenty-one days after the monthly periods; and while, on the one hand, I am quite

ready to admit a greater disposition to impregnation shortly after a menstrual period, yet I know of no facts to disprove the opinion that the human female is susceptible of impregnation at any time between her monthly periods. Look at an ovary, when you know that a fecundated ovum has been cast out from a Graafian follicle, and no matter when you examine it—whether a few days or a few months after its dehiscence,—there is a sure sign of its escape in the presence of a corpus luteum. Nothing can be more distinct or characteristic than this. And when ova have been detached at the œstrum of the lower animals, without sexual congress, corpora lutea have been seen in no respect differing from the ordinary corpora lutea. But at the so-called œstrum of the human female —at her menstrual time—I am quite convinced that no bodies, such as I know to be corpora lutea, are met with. And I infer from this that no ovum or ova have escaped.

Medical Gazette, July 13, 1849, *p.* 48.

123.—*On the Induction of Abortion by Puncturing the Ovum.*—By Dr. H. Oldham, Lecturer on Midwifery at Guy's Hospital.—The common plan which I adopt for opening the ovum by puncturing the membranes, is by employing an instrument which consists of a cylindrical silver canula, slightly curved at its further end, with a single opening, and two wire loops projecting from the sides of the other extremity, through which two fingers may be passed to hold and fix it. A movable stilette, tipped with a sharp point like a trocar, and so cut that the point does not touch the sides of the cylinder when the stilette is run through it, can be made at option to pass beyond the end of the canula. The way to use it is this:—Place the patient, if you like, on the left side, run up the index finger of the left hand to the os uteri, and if possible *within* it; then guide the instrument with the point of the stilette concealed in the canula, on the finger, within the uterus, and carry it forward until you feel that there is some resistance; then press the point of the stilette against the membranes and pass the canula a little further on. Then withdraw the stilette and let the liquor amnii flow through the canula. It is a good precautionary point to prevent the waters breaking forth too impetuously, because in doing so, either a loop of funis, or a hand, or even a leg (as I have once known), may be washed down by the side of the presenting part, and complicate the case.

You can do much more with the body of the womb with the patient on the back than on the side. Your two hands can act so much better in concert: you can impress the contents of the womb, and really, in some cases when the fluid contents of the ovum are large, as is the case in the fourth month to the sixth, you can sometimes gather the fœtus up to one part; and again, by pressure downwards, you can make a column of the liq. amnii press out the membranes, rendering them more tense, and so help their puncture.

You may learn a great deal, both in midwifery and in the organic

diseases of the womb, by always examining *the body of the womb.* and not merely its os and cervix.

The *time* which elapses between the puncture of the ovum and the supervention of labour varies very much in different cases. Five or nearly six days is a long interval, the longest that in my own experience I have known. The period of gestation probably has something to do in retarding it.

Before quitting this matter of puncturing the ovum in early pregnancy, I wish you to bear in mind that a few weeks of development makes a great deal of difference in the anatomical relations of the ovum. I punctured the amnion in one case in the eighteenth week, when the ovum well fills the cavity of the uterus: but supposing I had done so between the second and third months: well, in that case, if you examine this preparation (an impregnated uterus at the second month), you will see that the ovum, covered by its vascular reflex decidua, occupies only a part of the cavity of the uterus, and that there is a second large cavity, if I may so say, which, as it is lined entirely by the uterine and reflex decidua, is properly enough termed the decidual cavity. Now, this cavity is of great importance to the early ovum. It holds an embryotrophe, which is secreted by the glands of the uterine decidua, and is conveyed to the ovum through the cells of the reflex decidua. It is thus a source of nutrition to it—a kind of supplementary digestive sac. Beside this, it has a mechanical action, and holds off the dense uterine walls from the growing ovum, so as to allow the latter to expand, which at length it does to the entire obliteration of the decidual cavity. Well, then, what would you do if you passed a sound into such a uterus as this? You would, of course, unless the placenta was over the os uteri, at once open this cavity. And now comes the practical point—Would this be the equivalent of tapping the ovum? The cavity would necessarily be emptied by the fluid flowing out, which would accomplish two things:—1st. It would withdraw from the ovum an important source of its nutrition, and 2ndly, the walls would sink and contract upon it, which would go far to flatten or crush it; and I cannot but think that this would generally be sufficient to cause abortion. At any rate it is a lesser operation, which may well be attempted at first. If you are obliged to puncture an ovum at a very early period, it requires great care lest you injure and inflame the uterus; and also, by lessening the already small ovum, and destroying its shape as an oval sac, you impose a far greater difficulty on the womb to cast it off. Indeed, the collapsed membranes may be retained for some days or even weeks, and be cast off piecemeal, with fœtid discharges and recurring hemorrhages; and the female, by the time the uterus is cleared, be left in a feeble and exhausted state. I believe, if you have ever to conduct a responsible case of this kind, (I am speaking of an early period, say the sixth month), it would be better for you to be content with opening only the decidual cavity, and excite the uterus by other means, or postpone, if possible, the puncture of the ovum until the sixteenth week.

Medical Gazette, July 13, 1849, *p.* 51.

124.—*Case of Abortion produced by Metastasis in Cynanche Parotidœa.*—By T. SALTER, Esq., F.L.S., Poole.—[Mr. Salter's patient was a lady about twenty-five years of age, who was attacked with cynanche parotidœa when advanced just beyond the third month of her third pregnancy. (She had gone to the full time in her previous pregnancies.) The usual treatment was adopted. After a day or two of vaginal discharge, uterine pains and hemorrhage came on suddenly in the night, and a fœtus was discharged. The hemorrhage continuing, Mr. Salter gave ergot of rye, and adopted other proper treatment, and in fifteen hours after the birth of the fœtus, he was enabled to remove the placenta. Mr. S. says:]

I was much surprised at the appearance of the placenta, to which organ it had very little apparent resemblance: it was about the size of my two closed hands, and had the look of a large coagulum; and had not about two inches of the torn funis remained connected with its centre, it might readily have been mistaken for such. On closer inspection, however, the amnion was observed to be extended over the fœtal surface of the organ, and the decidua over the opposite or maternal aspect: between these two membranes, and within the cellular structure of the placenta, a large mass of blood, which constituted the chief bulk of the secundines, was contained. The resemblance to a coagulum consisted not merely in its external aspect, but a section of the mass, which was an inch and a half in thickness, exhibited an equally apparent likeness to coagulum. There was, however, a small portion, about an inch square, in a normal state; but it was only by maceration, and squeezing out the blood, that the other and larger division was brought to exhibit the characteristics of true placenta.

Remarks.—Among the various causes of abortion, hemorrhage into the cavernous structure of the placenta is the one of most frequent occurrence. Dr. Lee, in his lectures on this subject, published in the Medical Gazette, observes—"By far the most frequent cause of the accident is a morbid condition of the ovum;" and subsequently he goes on to say—"out of fifty-six cases of abortion, in four only was the ovum in a healthy condition."

Hemorrhage into the placenta would appear to occur under two forms, which may severally be denominated acute and chronic. The case here narrated clearly belongs to the former division, and was probably the result of an undue momentum of blood to the uterus, leading to rapid hemorrhage, and the consequent formation of the large and uniform coagulum discovered in the cavernous structure of the placenta.

The chief point of interest in the foregoing case is the question, how far the *specific* influence of the cynanche parotidœa might have been operative in causing abortion? It must be granted that the mere febrile excitement of any severe inflammatory disease is enough, in persons so disposed, to excite abortion; but I much question if this case can be so accounted for, and that the abortion was produced simply by the febrile disturbance of the disease,

independent of its specific action. Maintaining, therefore, the idea that the accident was the result of metastasis, it appears to me interesting *physiologically*, as it would tend to show that the ovum, while in the uterus, maintains the same relations to the rest of the organism, and is subject to the same influence, as when in the ovarium; and *practically* it would have an important influence on prognosis, which I should think, as far as the ovum is concerned, very unfavourable in such cases.

We have abundant experience to show, through the influence of the disease, the relation subsisting between the parotid glands, whilst affected with this specific inflammation, and the testicles in the male; and also between the same organs and the breasts in the female: and from the known sympathetic association of the mammary glands with the ovaria and uterus, it may readily be supposed that in cynanche parotidœa, an irritation may be communicated to the uterine system leading to results incompatible with the continuance of the process of utero-gestation. And, on the other hand, it is possible, in this chain of causation, in cases where the female breasts are affected in cynanche parotidœa, that the influence on them may be secondary to some primary disturbance of the ovaries, though this may not be disclosed by any palpable symptoms referred to the latter organs themselves. The analogy between the testes and the ovaries, and the sympathy of the breasts with the ovaries, are in favour of this hypothesis.

Medical Gazette, July 20, 1849, *p.* 106

125.—*On the Treatment of Sterility by Catheterizing the Fallopian Tubes.*—By Dr. W. Tyler Smith.—[Dr. Smith states that since he commenced his investigations, he is more than ever convinced that the mechanical causes of sterility have been very much under-rated. He gives the following directions for using the sound:]

In operating, the uterine sound should always be passed to the fundus uteri, in the first instance. I have had a sound made which answers much better, for all purposes for which this instrument is required, than the one in general use. The common sound is rounded and knobbed at the uterine extremity, and it is notched on its posterior surface, to enable the practitioner to measure, with the finger, the distance it has entered into the os uteri. Consequently it is generally used in the dark. I consider that the sound should seldom if ever, be used blindly, and without the speculum. The adaptation of the present instrument for use by the unaided touch, has led to serious mischief. Two unpublished cases have come to my knowledge, in which it has been thrust into the gravid uterus, —the gravid uterus in one case being mistaken for retroversion. The sound, therefore, should be graduated on the anterior surface, so as to be used by sight as well as touch. I have had a further alteration made in the uterine extremity, the end being flattened instead of rounded. The uterine canal is flattened in the natural

state, and there is often a difficulty in passing the knob of the common sound through the os internum. On withdrawing it, a blood-stain is frequently observed upon the metal. This difficulty is lessened by having the sound flattened somewhat like the female catheter, to accommodate it to the flattened entrance and canal of the uterus. This necessity for a flattened instrument for the uterus is recognised in the construction of the intra-uterine pessary. I was led to adopt it in the sound from observing, that in some cases, when the knobbed sound passed only with considerable difficulty, the Fallopian catheter, equal to it in width, but not in depth, passed with greater readiness.

[Dr. Smith has had some Fallopian bougies made by Weiss, of such extreme fineness that he thinks it almost impossible to injure the parts with them.]

Lancet, Aug. 4, 1849, *p.* 116.

126.—REMARKS ON DR. TYLER SMITH'S OPINIONS RESPECTING THE STIMULUS TO THE UTERINE ACTION IN PARTURITION.

By one of the Reviewers in the British and Foreign Medico-Chirurgical Review.

[We avail ourselves of one of the ablest articles which has lately appeared in the British and Foreign Medico-Chirurgical Review, to place before our readers some comments upon Dr. Tyler Smith's views on the processes of menstruation and parturition. But we advise the reader to go to the No. for July, 1849, for a more complete insight into the Reviewer's opinions on this important subject. First, as to *menstruation*, the Reviewer makes the following remarks upon the modern theory of menstruation, (that which "makes it dependent on the periodical excitement of the ovaria, and the escape of mature ovules from the Graafian vesicles,") which is adopted to its full extent by Dr. Smith.]

No one, we believe, has made a larger series of observations on this subject than Dr. Ritchie, of Glasgow; and he distinctly states, as the results of his inquiries,—1. That even during the period of childhood, long before the period of catamenia, there is a continual rupture of ovisacs and discharge of ova at the surface of the ovarium. 2. That although, after the establishment of the catamenial flow, the discharge of ova takes place *most frequently* at the menstrual period, the two occurrences are not necessarily coexistent; for menstruation may take place without any such rupture; whilst on the other hand, the maturation and discharge of mature ova may take place in the intervals of menstruation. These *facts* are alluded to by Dr. T. Smith as, "*aberrations of the ovular theory of menstruation;*" and he thus expresses himself in regard to them:

"The conclusion I should draw from Dr. Ritchie's papers is,

that in certain cases the ovarian action of menstruation is incomplete; that though it excites the catamenia, it is insufficient to cast off an ovule. The ovules shed in the intervals between the catamenial periods, without any uterine discharge, are probably either immature, like those of childhood, or they may escape from the ovaria without a sufficient degree of ovarian excitement to set up the catamenia, being in this respect the antithesis of those other cases in which there is uterine secretion without the extrusion of ova from the ovarium. In perfect menstruation, both ovarian excitement, and the escape of an ovule concur; but the ovarian excitement is absolutely indispensable."

It is Dr. T. Smith's object to show that the uterine excitement is dependent upon the ovarian; the catamenial secretion from the uterus being the result of a sympathetic action propagated from the ovaries. To us, on the other hand, it appears that the two phenomena are in some degree independent, though closely linked together, as are other phenomena having a common purpose. The ovarian excitement tends to the maturation and discharge of ova; the uterine, to the formation of a decidua. The former may never be perfected, and yet the latter may completely take place. On the other hand, the former may take place effectually, and yet the latter may not occur in any degree consentaneously. Again, the phenomena of suspended and of vicarious menstruation appear to us to indicate that the periodicity is not simply ovarian but constitutional; the periodical loss of a certain part of the contents of the sanguiferous system being required, for the maintenance of the healthy condition of the system at large. Fully admitting that in a large number of cases, amenorrhœa is rather the result than the cause of the general disorder, we yet think it impossible to contemplate the results of the suspension of normal menstruation by some local cause, without coming to the conclusion that, whether the menstrual flux consist of pure blood, or of certain of its constituents more or less altered, its removal from the sanguineous current is required to keep the latter in its due condition. This we can understand, even if these constituents should be no others than those adapted to become organized into a decidual membrane; since, as Mr. Paget (following Treviranus) has pointed out, the constituents of any organized tissue stand to the remainder of the body in the condition of materials destined for excretion.

We are disposed, then, to regard the uterine excitement as by no means so entirely dependent upon the ovarian as our author would have us believe. Although usually consentaneous, either *may* occur independently of the other; and there is no constant correspondence between the degree in which the two organs respectively are affected. Here, as elsewhere, we see the necessity of attending to the *exceptional* cases. To put them aside as mere "aberrations of a theory," is obviously unphilosophical. No theory can be complete which does not include the unusual as well as the ordinary phenomena; and it frequently happens that the greatest amount of information is obtained respecting the conditions of the latter, from

the careful observation of the former.—In his zeal for the prerogative functions of the ovaria, Dr. T. Smith goes so far as to assert his conviction that the greater part of the pain in dysmenorrhœa consists in ovarialgia; "the deep lumbar pain being decidedly ovarian and not uterine." Dr. T. Smith's experience of this disorder must, we apprehend, be rather limited, or his observations must be very much swayed by his preconceived hypothesis; for many a woman can assure him that her monthly pains bear a close resemblance to those of the first stage of labour; and he will not, we presume, assert that *these* are entirely ovarian. "Uterine disturbance must be considered as a secondary condition—an aggravated symptom of ovarian excitement in painful menstruation." He is, "decidedly opposed to the view that dysmenorrhœa is *caused*, in the majority of cases, by chronic inflammation of the os and cervix uteri. The relation between dysmenorrhœa and inflammation of the os uteri is one of coincidence, not of causation, nay, it is often a symptom induced by ovarian irritation, a symptom requiring palliation, but the relief of which by no means constitutes the whole of our treatment." We would not go so far as to assert that the cause of dysmenorrhœa is to be found in the inflamed or strictured condition of the os uteri "in a majority of cases;" but that in a large number of instances, the violent pains and spasms attending the return of the catamenial period are directly consequent upon the morbid condition or malformation of the uterus, is proved by the immediate relief which the judicious treatment of these conditions affords. If ovarian disorder participate at all in the general distress, it must therefore be as a secondary consequence of the undue uterine excitement.

[The reviewer next proceeds to discuss Dr. Smith's opinions respecting the origin of the parturient actions. These opinions are stated in the following extract from Dr. Smith's work:]

"I believe that at the time of parturition in mammalia, the uterus and the uterine nervous system are excited by the ovaria; that it is the ovarian excitement which induces both the permanent contraction of the uterus immediately before the coming on of labour, and the tendency to those reflex, emotional, and peristaltic actions, by which parturition is completed. In menstruation, a small synergic and reflex arc is described between the ovaria and the Fallopian tubes: in parturition, a larger arc is in operation, extending from the ovaria to the uterus. According to my researches, the excitement of the uterine nervous system at parturition, upon the presence of which the due performance of this function depends, is caused by ovarian excitement. At the time of ordinary menstruation, the ovarian irritation which excites the contraction and rigidity of the Fallopian tubes is manifest. Throughout utero-gestation, the ovarian excitement returns in a slight degree at each periodic date; but at the eleventh period after conception (reckoning the last catamenial period inclusively) the ovarian

excitement returns in full force, and, as a consequence, the uterine excitability and the uterine actions of labour begin."

According to Dr. T. Smith, therefore, the muscular excitability of the uterus at the full term is dependent upon the ovarian excitement; and the occurrence of its contractions on the 280th day (or thereabouts) after conception is a reflex phenomenon, for which the exciting cause is to be found in the eleventh periodical access of ovarian excitement. We quote the following passage from the Appendix, in order to show how much of this doctrine is regarded by Dr. T. Smith as his own, and how far he considers himself to have been anticipated in it by those obstetricians, who have held similar views with himself as to the coincidence between the ordinary duration of pregnancy and ten menstrual periods:

"The following extract from Dr. Fleetwood Churchill's work on the 'Theory and Practice of Midwifery,' shows how perfectly the idea of the regular duration of pregnancy, and the relation of parturition to the catamenial periodicity, may exist, without any idea of the cause of labour. Dr. Churchill is perfectly conversant with 'the fact,' that the average duration of pregnancy is 280 days, or a time equalling ten catamenial periods; and he suspects that labour comes on at the tenth period after impregnation; but the 'exciting cause' is quite undiscovered. It is the knowledge of an excitor spinal nerve in the ovarium which is the one great point wanting, and without which the mere fact of periodicity is confused and of little value. I insist upon this point strongly, because attempts have been already made by those who confound *cause* with mere *coincidence* and *time*, to set aside my claim to whatever merit may lie in the detection of the cause of labour. The passage I quote proves that it is quite possible to have a tolerably accurate notion of the 'utero-ovarian periodicity' of pregnancy, and yet to consider that 'all search has hitherto failed in discovering the exciting cause of labour.' Obstetricians might have gone on, as they had done before, from generation to generation, repeating to themselves the fact, that the duration of gestation is a multiple of the catamenial period, without any conception of the ovario-uterine nervous arc, which excites the uterus to parturient action."

[The reviewer objects to this, that Dr. Smith has not shown either upon anatomical or experimental evidence, the existence of the "ovario-uterine nervous arc." He observes that]

He has not proved that any more direct communication exists between the ovaries, the uterus, and the spinal cord, than that which is established through the spinal fibres intermingled with the sympathetic; whose endowments differ so much from those of ordinary spinal nerves, that any arguments founded upon the analogy of the latter are unsafe in their application to the former. He has not shown that irritation of the uterine nerves will at any time excite parturient actions in the uterus. He has not shown that section of these nerves prevents the access of the parturient

actions at the usual period. Still less has he shown that irritation of the ovarian nerves will excite uterine contractions by reflex agency; or that section of the ovarian nerves, by interrupting the continuity of the "ovario-uterine nervous arc," will prevent the access of the parturient efforts of the uterus at the regular period. All this he must demonstrate experimentally, before he can be admitted to have established his position, that the ovarian nerves stand in the same relation to the act of parturition as do the pneumogastric to that of respiration. Not one particle of evidence on this subject is adduced by him, except that afforded by the experiments of MM. Brachet and Segalas, of which we have already disposed. Before leaving this part of the inquiry, we must remark that Dr. T. Smith seeks to establish a relation even more intimate than that to which he refers for illustration; for every physiologist knows that there are many excitors to reflex action in addition to the pneumogastrics; so that section of the latter diminishes the frequency of the respiratory movements, but does not suspend them altogether; whereas Dr. T. Smith refers so exclusively to the ovarian nerves as the excitors of parturition, that this action would seem, in his view, to be entirely dependent upon the stimulus which they supply.

The evidence upon which Dr. T. Smith relies, is, to our minds, of far too vague and unsatisfactory a nature to be admitted as *establishing* a principle of such importance, even if there were no opposing considerations of any weight. It consists of little else than a reiteration of the facts and arguments he has already adduced respecting the coincidence between uterine action and ovarian excitement; together with other facts, the relevancy of which we cannot admit.

[The following evidence is adduced by Dr. Smith in support of his views, derived from the phenomena of human parturition.]

"The duration of human pregnancy is well known to be a multiple of a catamenial period. It has also been observed that, in rare cases, where the duration of pregnancy exceeds the ten menstrual periods, the function of parturition is deferred to the following period, so as to make pregnancy reach to eleven periods inclusive. This fact came out most clearly in the obstetric evidence on the Gardner peerage case, and will prove of very great importance in cases of supposed criminality in wedlock, or in establishing the legitimacy of posthumous children. On the other hand, it is well known, that when utero-gestation is brought to a premature termination, it is at what would have been a menstrual period that abortion usually takes place. There is, in fact, in all women a tendency to abortion at the times represented by the catamenial periods. In placenta prævia, the hemorrhage occurring in the later months of pregnancy generally happens at the periodic dates. Hemorrhage sometimes occurs for two or three periods in succession before parturition in these cases, with perfect immunity in the intervals. The occurrence of uterine pains at the termination of the natural

period of gestation, in cases of extra-uterine fœtation, points very decidedly to the operation of some extra-uterine source of excitement. After abortion—and very speedily too, in some cases,—it is known that there is a remarkable aptitude to conceive; and during the catamenial climacteric, when the ovaria are irregularly excited, there is frequently an increased tendency both to conception and abortion. These accumulated facts are sufficient to establish the influence of the ovaria and of the ovarian function on the uterine phenomena of gestation and parturition. But to complete the evidence, we must inquire what proofs there are that the sexual excitement which attends exaltation of the ovarian irritability in the lower animals, is present in the human subject. An attentive observer will not fail to perceive the existence of this excitement, though in a rudimentary degree. I have insisted in a former lecture, that there is an actual increase of the sexual emotion during, or immediately after, the catamenial periods. There are also distinct traces of sexual excitement in some cases of parturition. That they are not always present does not tell against my argument, because the reasons why they are not so present are very evident."

Dr. T. Smith then goes on to state objections to the use of anæsthetic agents in parturition, on the ground of their removing the moral restraint which ordinarily keeps these feelings in check; and then returns to his position that "parturition does not merely occur at what would otherwise be a menstrual period, but *is essentially a menstrual period.*" The "show" is regarded by him as the equivalent of the access of the catamenial discharge; the lochial flow as its continuation, altered by the admixture of discharges from the placental site. He refers to the lochial flow which occurs after abortion in the early months of pregnancy, as indicating that it is something else than a discharge from the placental site; and he states that having examined the internal surface of the uterus after death in parturition, he has seen the whole of it dyed with the sanguineous secretion.

If Dr. T. Smith's doctrine were rigorously true, the duration of pregnancy ought to be 280 days in those cases *only* in which conception takes place *immediately before* the regular period for the return of the catamenia; since the commencement of parturition answers, according to him, to the commencement of œstruation. In those cases, usually considered to form the great majority, in which conception takes place at an early period *after* the cessation of the catamenia, if labour comes on at the eleventh menstrual period, the intervening time must be 280 days *less* the time which elapsed between the date of conception and the commencement of the antecedent menstruation. Thus, for example, if the menstrual flux usually occupy five days, and conception take place on the sixth from its commencement, the eleventh recurrence of the menstrual flux, which ought, on Dr. T. Smith's hypothesis, to fix the date of parturition, will fall on the 280th day from that commencement, but on the 274th day from the date of conception; in other words, the duration of gestation would be only 274 days. Now we

knew a case in which the date of two conceptions could be exactly ascertained. The first took place on the day before the catamenial period; the menstrual flux appeared at the usual time; but its duration and amount were greatly reduced. Labour supervened on the 280th day from the date of conception; that is, at the time when the eleventh catamenial period would have commenced. But in another pregnancy in the same individual, the date of conception was the fourth day after the cessation of the catamenial flow, and this had lasted seven days; nevertheless labour supervened as before, on the 280th day, that is *eleven days later* than the time for the eleventh recurrence of the catamenial period. This is not an exceptional case. We are confident that the general experience of accoucheurs will accord with our own, in the conclusion that where the date of conception can be exactly ascertained, labour may be expected to commence on the 280th day, without any reference to the question whether or not the conception took place before or after the catamenial period, which may be considered, on the average, to make a difference of at least a week. We have not found any reference to this difficulty in Dr. T. Smith's discussion of the subject; in fact, in his 'Periodoscope,' he unhesitatingly assigns 280 days as the regular term of gestation, and gives directions for determining the various periods of expected abortion, &c., all of which are to date from the day of conception,—as if it were a matter of perfect indifference whether that conception had occurred before or after the menstrual period.

We have tested the correctness of Dr. T. Smith's hypothesis by the ordinary experience of obstetric practitioners. We shall now test it by exceptional cases, which afford still stronger evidence of the want of that *necessary* connexion between the two phenomena in question, which alone can give to them the relation of cause and effect. Every accoucheur is acquainted with instances in which the menstrual period is something different from 28 days; for instance, 21, 25, or 30. Now if the term of gestation be necessarily a multiple of that period, it ought not to be affected by any such irregularity. Of such a modification, however, we have no evidence; but on the other hand, we have much proof to the contrary. In the case we have just cited, the catamenial period is only 26 days; the menstrual flux returning with the usual regularity at this shortened interval. Now if the uterine action in parturition were dependent upon the recurrence of œstruation, the term of gestation in such a case ought to be 260 days; or, if that term be exceeded, it ought to be protracted to another period of œstruation, in which case it would be 286 days; yet in three pregnancies it has been 280 days, as usual. We could cite many other cases of the same kind; all of them equally tending to support our position that the uterine act of parturition is independent of ovarian œstruation; and that, where the two occur simultaneously, the relation is one of coincidence merely.

Another proof to the same effect is derivable from the phenomena of protracted gestation. Dr. T. Smith affirms, in the passage we

have already cited, that "in the rare cases where the duration of pregnancy exceeds the ten menstrual periods, the function of parturition is deferred to the following period, so as to make pregnancy reach to eleven periods inclusive. This fact came out most clearly in the obstetric evidence on the Gardner peerage case." We could scarcely cite a stronger instance than this of Dr. T. Smith's rashness of statement, and disregard of all facts but such as tend to confirm his own hypotheses. It must be remembered that in the Gardner peerage case, the object of those who sought to establish the legitimacy of the offspring was to show that pregnancy might be protracted to 311 days, or 31 days beyond the usual term: and in the medical testimony that was adduced to prove the possibility of such an extension of the usual term, any minor degrees of prolongation would of course be less attended to. Yet it will be obvious we are sure, to those who will go through the evidence given on that case, that there is nothing even there to justify the assertion, that pregnancy, if protracted at all beyond the tenth catamenial period, extends to the eleventh. In the table published by Dr. Merriman, who was one of the witnesses upon that occasion, it is expressly stated that whilst 33 women were delivered in the 40th week, 22 were delivered in the 41st, 15 in the 42nd, 10 in the 43rd, and only 4 in the 44th. Yet, according to Dr. T. Smith's assertion, all the women who passed the 40th week ought to have retained their ova until the 44th. Nature, however, refuses to tie herself down to such rules. Dr. Murphy, in his 'Report of the Obstetric Practice of University College for 1844,' gives a similar table, in which it is stated that whilst 24 women were delivered in the 39th week, 25 in the 40th, and 32 in the 41st, 25 were delivered in the 42nd week, 19 in the 43rd, 9 in the 44th, and 11 in the 45th. We believe that all these cases are open to the objection that the date of conception was not known, and that impregnation was presumed to have occurred immediately *after* the last recurrence of the catamenia; whereas it might have taken place just *before* the next œstruation, which appears to be the case with those Jewish females who observe the Rabbinical law of abstinence from sexual intercourse for five days beyond the period laid down by Moses. This would cut off about three weeks from the presumed period of gestation, and would probably tend to reduce a large proportion of the cases of supposed protraction to the regular term. But it will not do so by any means in all cases; and moreover we have an amount of trustworthy evidence, derived from cases in which the day of conception could be fixed, and in which there was no motive for falsification, sufficient to establish the possibility of protraction beyond the usual term, for *any number* of days or weeks *less* than the additional month; how much *more* may be allowed, can scarcely be considered as yet decided.

The evidence of comparative gestation is all to the same effect: and we cannot refrain from asking why Dr. T. Smith has kept it back, when he is evidently acquainted with at least that part of it supplied by Tessier, and himself contends that, "a comparison be-

tween human and comparative parturition. and all the other forms of parturient action, is essentially and absolutely necessary to a comprehension of this branch of human physiology." If it be admitted that we have a right to reason from the one case to the other, it is obvious that the class of phenomena in question affords most satisfactory evidence in regard to variability in the term of gestation; since the date of conception can be more positively determined than is usually possible in the case of the human female. In addition to the observations of Tessier, we have now a long series carried on upon the cow, under the direction of the late Earl Spencer; from which it appears that, the average term of gestation in that animal being 284 or 285 days, an excess of a few days is not at all uncommon; whilst a prolongation to the extent of a fortnight or three weeks is much more common than an extension to a month. We shall presently speak of one very curious part of these observations; from which it appears that the *male* parent exerts an influence over the term of gestation, a phenomenon altogether inconsistent with Dr. T. Smith's hypothesis.

Having now tested the value of Dr. T. Smith's hypothesis by applying it to three different classes of facts which are altogether unnoticed by him, and having found it deficient even in the attribute of probability, still more in any claim to be received as a demonstrated truth, we might leave the question where we found it, and say, as many have said before us, that no account whatever could be given of the ordinary limitation of the term of gestation to 280 days. Even if no valid explanation can be given, however, it seems to us that it will be useful to know in what direction we are to seek for one; at any rate, it cannot but be advantageous to point out certain facts which any theory on the subject ought to embrace.

In the first place, then, it must be admitted on all hands that there is a certain disposition or tendency, on the part of the uterus, to disburthen itself of its contents at the termination of the fortieth week of gestation, rather than at any other time. Even if we grant, with Dr. T. Smith, that at each period of œstruation during pregnancy there is a tendency to abortion, we have still a right to ask why, in such a vast majority of cases, the fœtus is retained until the tenth of these periods has elapsed, and is then ejected? Why should not labour take place just as readily at the *tenth* as at the *eleventh* œstrum; and why it should it be so very seldom deferred until the *twelfth*? We cannot see that Dr. T. Smith can give any other answer to these questions, than that the uterus and its contents are then in such a condition, as to render the ovarian stimulus far more effectual at that time than at any other. In assigning the ovarian stimulus, then, as *the cause* of parturition, Dr. T. Smith has altogether left out of view the fact that this cannot operate alone; and that unless there be, coincidently with this, a certain peculiar readiness on the part of the uterus to part with its contents, labour would no more come on at the end of the tenth monthly period than at the close of the ninth. As he has not shown whence this arises, we must consider him as having advanced but

little in the explanation of the causes of parturition, even if we concede to him all that he demands. The act of parturition was likened by Buffon to the dropping of ripe fruit; and we believe, that in seeking for its cause partly (to say the least) in that condition of the uterus and its contents which may be designated as *maturation*, we are justified by all the facts at present known to us. The phenomena of the separation of the offspring from the parent, as a consequence of the completion of certain stages of growth on the part of the former, the loosening of the connexion between them being a part of this sequence of changes, are seen very extensively through the organized world; and they are obviously analogous to the phenomena of separation and detachment, which occur in the parts of an integral structure during its evolution. No one has any difficulty in understanding that the fall of the leaf is due to the death of the tissue which connects it with the stem; or that the opening of a flower is the result of the turgescence of its tissue, consequent upon its full development. Every one knows that there are flowers whose first opening takes place with great regularity at a particular hour; we have ourselves repeatedly witnessed this in a West Indian species of amaryllis, the long floral leaves of which became detached from each other, and sprung apart, within a few minutes before or after six o'clock in the evening. Although many vegetable and animal periodicities may be shown to be under the influence of external agencies to a certain extent, yet there is ample proof that they are essentially independent of such agencies, and that they form parts of that cycle of phenomena, of which the whole life of the being, from its origin to its final decay, is made up. We do not see anything more wonderful in the recurrence of certain changes at particular times, than in the progressive evolution of the entire organism. The exuviation of the milk-teeth, for example, in a certain regular succession, and at such uniform times, as to afford one of the best tests of the age of the individual, is a phenomenon which strongly marks the disposition in the human system to the spontaneous severance of a vital connexion which has served its purpose, and to the detachment of the part in which it has ceased to exist.

We have adequate reason to believe that the embryo of warm-blooded animals is extremely regular as to its term of development. Thus, in the common fowl, we may calculate almost to an hour (provided that the eggs have not been detained in the oviduct of the hen) the time when the chick will emerge from the shell. And it is well known that the stages of progressive development in the human fœtus afford indications from which its age can be determined, with a pretty close approximation to truth. That the development of the uterine structure goes on *pari passu* with that of the contained fœtus, appears equally evident from a consideration of its phenomena; especially those attending the formation of the placenta, in which organ there is such a marvellous commingling of parts, formed from two distinct and independent centres of development. Now we see pretty clear indications that the placenta of the

fully-developed fœtus is somewhat in the condition of the footstalk of a ripening fruit; that is, that having attained its full evolution as an organ of temporary function, its connexion has a tendency to become dissevered, in virtue of causes inherent in itself alone, and quite independent of external agency. Something of this kind seems to have been recognized by Dr. T. Smith himself, who expresses himself, in his chapter on abortion, to the following effect:

"The placenta has a tendency to become unfit for fœtal respiration towards the end of utero-gestation, when nature is preparing for the change from branchial to pulmonary breathing. There are frequently observed on the surface of the mature placenta crystals of carbonate of lime, which must tend to interfere with its functions as a respiratory apparatus, and generally, I believe, to facilitate its separation from the uterus. This caducous preparation of the placenta, by the deposition of the salt of lime, is probably connected with the demand for ossific matter in the fœtus; but it must also remind you of the deposit of lime upon the egg of the bird, or of silica in the stems of ripe fruit, to facilitate its separation from the parent tree; or we may compare it to the deposit of earthy salts in the lungs of old age, as preparatory to the death of the individual. It is pretty certain that, in some cases of abortion in the latter months, caused by the death of the fœtus, the death has depended on the low respiring power of the placenta, the placental development having progressed so rapidly as to render the organ prematurely deciduous."

Another indication of the necessity for this maturation is to be found in the difficulty with which uterine contractions are excited by the ergot of rye before the normal time for parturition has arrived. Perhaps the strongest proof, however, that the parturient contraction of the uterus is in some way dependent upon the condition of the placenta, is derived from the phenomena which have been described under the designation of *superfœtation*. In whatever way we explain these phenomena,—whether we regard the two fœtuses as the products of two distinct conceptions,—or whether we regard them as twins, of which one is more advanced in its development than the other,—the facts remain the same; namely, that one child may be expelled by the parturient efforts of the uterus, whilst the other is detained; that the uterine contractions do not detach the placenta of the latter; but that pregnancy may continue for many weeks or even months longer, until the second child has attained its full term of development. In some of these cases it is positively affirmed that the first child presented every appearance of maturity; in others, it appears that an abnormal condition of the placenta was the cause of its detachment. In all, however, it is obvious that the retention of the second fœtus was consequent upon *its* immaturity; and we do not see how this could influence the uterine contractions, except through the condition of the placenta.

There is another most important indication, that the immediate cause of the supervention of labour is to be looked for in the con-

dition of the attachment of the fœtus to its parent, rather than in ovarian excitement; namely, that preparatory changes are obviously taking place during the last fortnight, or thereabouts, of gestation. The uterus, as Dr. T. Smith himself tells us, begins to contract more closely upon its contents; and the disposition to relaxation in the soft parts surrounding the outlet of the pelvis manifests itself in the descent of the whole mass. We have no doubt that, during this period, a progressive change is taking place in the placental attachment; for such a change may be easily verified in the placenta of many of the lower animals (such as the cat), in which the fœtal and maternal portions remain more distinct than they do in the human female, these being separable from each other, as the period of parturition draws near, far more easily than at any previous time. And it is not difficult to understand that the completion of this change, involving (so to speak) the death of the entire placenta, shall be the immediate cause of the supervention of labour; just as we know that premature labour may result from the death or abnormal condition of the fœtus and of the fœtal portion of the placenta. This view affords us a rational explanation of the occurrence of uterine action in cases of extra-uterine fœtation, which is considered by Dr. T. Smith as clearly indicating that the exciting cause of labour is to be found elsewhere than in the uterus itself. For if the condition of the placental attachment furnish that cause, instead of the state of the ovary, it will do so equally, whether the placenta be attached to the lining of the uterus, or to that of the Fallopian tube, or to any other organ. As an additional indication that the exciting cause of labour has its place in the fœtal, rather than in the ovarian, connexions of the uterus, we may notice the fact, ascertained by Earl Spencer, that of 75 cows in calf by a particular bull, the average period was 288½ days; none of these having gone less than 281 days, and two-fifths of them having exceeded 289 days. That the male parent could produce any modifying influence on the ovarian periodicities of the female, cannot be thought probable for a moment; but that he should influence the term of fœtal maturation, must be admitted to be not unlikely.

In order to avoid the possibility of misconception, we shall briefly recapitulate our positions, in such a form as to indicate what we believe to be the true view of the subject, and to point out the chief errors into which we consider that Dr. T. Smith has fallen.

1. The muscular action of the uterus we regard as essentially peristaltic and independent of nervous stimulation. We fully recognise, however, the fact that the induction of this action is *often* brought about by nervous agency; but we consider this agency as operating, not directly upon the entire muscular structure of the organ (as when a voluntary muscle is thrown into contraction), but indirectly, by exciting its peristaltic movements, as in the case of other organs composed like it of non-striated fibre, and receiving, like it, their nervous supply from the sympathetic system. We altogether dissent from Dr. T. Smith's doctrine, that the parturient

contractions of the uterus, when once established, and even its tonic contraction anterior to labour, are excito-motor; because no adequate proof has been given that they are dependent upon nervous agency at all; and because all analogy, as well as the evidence of experiment and pathological observation (so far as they extend), go to prove that they are not. Nevertheless, we would not deny that the parturient actions *may be* excited in the first instance by nervous agency; it being very obvious, from the phenomena of abortion, that causes originating at a distance, and not capable of acting in any other way than through the nervous system, may bring about these actions. That the uterus may be thus affected through the nervous system, has long been taught by physiologists and obstetricians; and Dr. T. Smith's chief movement, in supposed advance, is in referring the regular parturient actions of the uterus to this source.

2. Dr. T. Smith maintains that the exciting cause of parturition is to be found in the recurrence of the periodical excitement of the ovary; which continues during the entire period of gestation, giving a special tendency to abortion at each return; and which, at the eleventh recurrence, acts with such potency as to induce parturition. He assigns no other cause, however, why this eleventh recurrence should be so much more effectual than the rest, than that by this time there is a much greater aptitude to contraction in the uterus itself, and a readiness to be thrown off on the part of the placenta. That this ovarian excitement, acting by reflection on the uterus through the spinal system of nerves,—the *ovarian* nerves being the *excitors*, and the *uterine* the *motors*,—is the exciting cause of normal parturition, is, we believe, an entirely original doctrine of Dr. T. Smith's; no others having advanced beyond the mention of the usual *coincidence* of the period of parturition with the eleventh ovarian nisus. From this doctrine, however, we feel called upon to withhold our assent; because, in the first place, no adequate proof has been afforded by Dr. T. Smith, that the coincidence of the two phenomena is in any way essential; and because, secondly, a number of phenomena, altogether left out of view by him, afford a clear indication to our minds that the coincidence is not essential, the normal parturient actions of the uterus being entirely independent of the ovarian nisus. We do not deny that abortion may sometimes result from ovarian excitement; but we conceive that this has no special action, and that it is only one out of many causes that may produce the result where a predisposition to it exists.

3. In searching for the real exciting causes of parturition, we conceive that there is ample evidence, both from the analogy of similar phenomena in the lower mammalia, and from observation on the human subject, to justify the expectation that it will be found in the condition of the placental attachment; and we can see no reason why the changes in which maturation consists, and on which, according to Dr. T. Smith, the peculiar efficiency of the eleventh ovarian nisus depends, should not, when complete, be

themselves as potent in exciting the first uterine contractions, as he supposes the ovarian stimulus to be.

4. The parturient efforts of the uterus having gradually advanced the presenting part of the child into the vagina, the other expulsor muscles are called into action through the spinal system of nerves; the requisite stimulus being supplied, either by afferent nerves of the uterus itself, as first suggested by Dr. Carpenter, or by those of the walls of the parturient canal, against the several divisions of which the presenting part of the child successively presses, as maintained by Dr. T. Smith. We are inclined to regard the latter as the correct view of the source of the action; but if any merit be due to the idea of the local origin of the stimulus which excites the reflex action of the expulsors, it appears to us that it may be fairly claimed by the first-named author.

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127.—ON THE INFLUENCE EXERTED BY THE PATERNAL UPON THE MATERNAL ORGANISM THROUGH THE MEDIUM OF THE FŒTUS IN UTERO.

By Dr. A. Harvey, Physician to the Aberdeen Royal Infirmary

Instances are sufficiently common among the lower animals, where the offspring exhibit, more or less distinctly, over and beyond the characters of the male by which they were begotten, the peculiarities, also, of a male by which their mother had at some former period been impregnated,—or, as it has been otherwise expressed, where the peculiarities of a male animal that has once had fruitful intercourse with a female, are more or less distinctly recognised in the offspring of subsequent connexions of that female with other males. A young chestnut mare, seven-eighths Arabian, belonging to the Earl of Morton, was covered in 1815, by a quagga, which is a species of wild ass from Africa, and marked somewhat after the manner of the zebra. The mare was covered but once by the quagga; and, after a pregnancy of eleven months and four days, gave birth to a hybrid which had distinct marks of the quagga, in the shape of its head, black bars on the legs and shoulders, &c. In 1817, 1818, and 1821, the same mare (which had in the meantime passed into the possession of Sir Gore Ouseley), was covered by a very fine black Arabian horse, and produced, successively, three foals, all of which bore unequivocal marks of the quagga. Several other examples illustrative of the general fact above stated will presently be given.

Great difficulty has been felt by physiological writers in regard to the proper explanation of this kind of phenomena. They have been ascribed by some to a permanent impression made somehow by the semen of the first male on the genitals, and more particularly on the ova, of the female: and by others to an abiding influence

exerted by him on the imagination of the female and operating on her mind at the time of her connection subsequently with other males, and perhaps during her pregnancy. But they seem to be regarded by most physiologists as inexplicable.

Very recently, in a paper published in the Aberdeen Journal, an intelligent veterinary surgeon, Mr. James M'Gillivray, of Huntly, has offered an explanation, which seems to me to be the true one. His theory is set forth in the following statements quoted from that paper:—"When a pure animal of any breed has been pregnant to an animal of a different breed, such pregnant animal is a cross ever after; *the purity of her blood being lost*, in consequence of her connection with the foreign animal;" and again: If a cow, "say of the pure Aberdeenshire breed, is in calf to a bull of the short-horn breed (known as the Teeswater breed), in proportion as this calf partakes of the nature and physical characters of the bull, just in proportion will the *blood* of the cow become *contaminated*, and herself a cross, for ever incapable of producing a pure calf of any breed." It is maintained, therefore (Mr. M'Gillivray adds), that the great variety of non-descript animals to be met with are the result of the crossing system; the prevailing evil of which is, the admission of bulls of various breeds to the same cow, *whereby the blood is completely vitiated.*

[A consideration of this subject, says Dr. Harvey, suggests the following questions:]

1st. Whether, in the case of a woman who has been twice married, and borne children to both husbands, the children borne to the second husband ever, or generally, partake of the peculiarities of the first husband.

2nd. Whether, in a family of several children, the younger children rather than the elder, are disposed, *cæteris paribus*, to exhibit the characters of the father.

3d. Whether a woman who has borne several children by the same husband, may not ultimately acquire some of the physical characters, or at least imbibe and manifest some of the morbid tendencies, of the latter.

[In treating this subject, Dr. Harvey proceeds first to state what facts are known regarding it, both as to the lower animals, and as to the human species.]

As regards the brute creation:—Besides the instance already quoted of the mare belonging to Lord Morton, there is another similar case recorded. A mare belonging to Sir Gore Ouseley, was covered by a zebra, and gave birth to a striped hybrid. The year following, the same mare was covered by a thorough-bred horse, and the next succeeding year by another horse. Both the *foals* thus produced were striped, *i. e.*, partook of the characters of the zebra, And it is stated by Haller, and also by Becker, that when a mare has had a *mule* by an ass, and afterwards a *foal* by a horse, the foal exhibits traces of the ass.

In the foregoing cases, the mares were covered, in the first instance, by animals of a different species from themselves. But cases are recorded of mares covered in every instance by horses, but by different horses, on different occasions—where the offspring partook of the characters of the horse by which impregnation was first effected. Of this Mr. M'Gillivray gives two examples. Thus, in several foals, in the Royal stud at Hampton Court, got by the horse *Actæon*, there were unequivocal marks of the horse *Colonel*, —the dams of these foals were bred from by Colonel the previous year. Again, a colt, the property of the Earl of Suffield, got by *Laurel*, so resembled another horse *Camel*, "that it was whispered, nay, even asserted at Newmarket, that he must have been got by Camel." It was ascertained, however, that the mother of the colt was covered, the previous year, by Camel.

It has often been observed, that a well-bred bitch, if she have been impregnated by a mongrel-dog, will not, although lined subsequently by a pure dog, bear thorough-bred puppies in the next two or three litters.

The like occurrence has been noticed in respect of the sow. A sow of the black and white breed (known as Mr. Western's breed) became pregnant by a boar of the wild breed, of a deep chestnut colour. The pigs produced were duly mixed, the colour of the boar being in some very predominant. The sow being afterwards put to a boar of the same breed with her own, some of the produce were observed to be stained or marked with the chestnut colour that prevailed in the former litter. And, on a subsequent impregnation, the boar being still of the same breed as the sow, some of the litter were also slightly marked with the chestnut colour. What adds to the value of the fact now stated is, that in the course of many years' observation, the breed in question was never known to produce progeny having the smallest tinge of the chestnut colour.

Breeders of cattle are familiar with analogous facts as occurring in the cow. A pure Aberdeenshire heifer was served with a pure Teeswater bull, to whom she had a *first-cross* calf. The following season, the same cow was served with a pure Aberdeenshire bull; the produce was a *cross* calf, which at two years old had very long horns, the parents both hummel. A pure Aberdeenshire cow was served, in 1845, with a cross bull—*i. e.*, an animal produced between a first-cross cow and a pure Teeswater bull. To this bull she had a cross calf. Next season she was served with a pure Aberdeenshire bull,—the calf was quite a *cross* in shape and colour.

Mr. M'Gillivray, after narrating the whole of the foregoing examples, says:—"Many more instances might be cited, did time permit. *Among cattle and horses they are of every day occurrence.*"

[The facts bearing on this subject in connection with the human species are much fewer in number. It is obvious that decisive facts can only be obtained by observation of marriages between parents of different races,—Europeans and Negroes, for instance. Some such facts Dr. Harvey records. He says,]

My colleague, Dr. Dyce, tells me, that he has certainly known one instance (if not more) where a Creole woman bore fair children to a white man; and that the same woman had afterwards to a Creole man other children, who bore much resemblance to the white man, both in features and complexion.

The following additional cases illustrative of this question have recently been communicated to me: the first by my friend the Rev. Charles M'Combie, of Tillyfour, minister of Lumphanan, in Aberdeenshire: the second by Professor Simpson, of Edinburgh:—

1. Mrs. ——, a neighbour of Mr. M'Combie, was twice married, and had issue by both husbands. The children of the first marriage were five in number; of the second, three. One of these three, a daughter, bears an unmistakeable resemblance to her mother's first husband. What makes the likeness the more discernible is, that there was the most marked difference, in their features and general appearance, between the two husbands.

2. A young woman, residing in Edinburgh, and born of white (Scottish) parents, but whose mother sometime previous to her marriage had a natural (mulatto) child by a negro man-servant, in Edinburgh, exhibits distinct traces of the Negro. Dr. Simpson, whose patient the young woman at one time was, has had no recent opportunity of satisfying himself as to the precise extent to which the Negro character prevails in her features; but he recollects being struck with the resemblance, and noticed particularly that the hair had the qualities characteristic of the Negro.

It is of more immediate interest, however, and of greater practical moment, to ascertain whether, through the medium of the fœtus, the husband may impart to his wife either the syphilitic virus, or the scrofulous diathesis, or any other constitutional morbid tendency (*e. g.* insanity) which he may possess. Facts are wanting on this subject; but it is not undeserving of patient inquiry.

Dr. George Ogilvie, of this city, informs me of a case, which fell under his own observation, where a woman was twice married, and had children by both husbands, and where the children by both marriages were scrofulous, although only the first husband had marks of that diathesis; the woman herself, and her second husband, being to all appearance quite healthy.

[Another case was communicated to Dr. Harvey, by Professor Pirrie, of Aberdeen.]

Mrs. H——, apparently perfectly free from scrofula, married a man who died of phthisis, She had one child by him, which also died of phthisis. She next married a person who was to all appearance equally healthy as herself, and had two children by him, one of which died of phthisis, the other of tubercular mesenteric disease—having, at the same time, scrofulous ulceration of the under extremity.

[Dr. Harvey proceeds to say, that, in these cases.]

Before the mother could have imparted the scrofulous taint to

her offspring by the second husband, she must herself have imbibed it from her first husband through the medium of his offspring while in utero. And, although still seemingly free of the taint, it may have required only the appropriate external conditions to call it into full activity in her own person. And, with regard to the syphilitic poison, there is no difficulty in understanding, and it is quite within the bounds of probability, that the fœtus, if contaminated with it by its father, may convey it to the mother. Messrs. Maunsell and Evanson, after mentioning that they have notes of the case of a syphilitic child, whose mother had been infected by a former husband (they do not say in what way)—and to all appearance cured five years before its birth—the father of the child (her second husband) being in good health, state that their experience would enable them to adduce many curious facts bearing on the communication of the syphilitic poison. Perhaps their experience might furnish an affirmative solution of the question at issue. It has been affirmed, indeed, that a man who has once had syphilis, but been seemingly cured of it for many years, may yet so retain the taint of it as to contaminate his offspring, without, at the same time, tainting his wife. Very possibly. But this does not prove that he may not contaminate his wife also; and the observation itself is in that respect fallacious, inasmuch as, in any given case of the kind, the wife may really have imbibed the virus, although in a latent form, and might subsequently give proof of the reality of the fact by tainting the offspring begotten by another and a perfectly healthy husband. Adopting this view, it may be found of importance, in contemplating marriage with a widow, to inquire into the constitutional peculiarities of her deceased husband!

[Dr. Harvey next discusses the theories which have been put forth in explanation of facts like these. He says]

The first is that suggested by the great Haller, who ascribes it to a permanent impression made by the semen of the male on the genitals, and more particularly on the ova, of the female; the second, that suggested by Mr. M'Gillivray, who ascribes it to an influence exerted by the fœtus in utero on the constitution of the mother. The notion entertained by Sir Everard Home and others, that it is an affair of the imagination, seems too absurd to require serious consideration.

Mr. M'Gillivray's theory seems to me to meet the whole facts of the case, and to derive support from a great variety of facts in regard to the reception and constitutional effects of morbific poisons and morbid diatheses.

Mr. M'Gillivray, indeed, supposes that there is a *direct* vascular connection between the fœtus in utero and its mother; and he seems to consider the validity of his theory to hinge on this assumption. The assumption, however, is untenable, nor is it at all necessary for the establishment of the theory.

[Dr. Harvey proceeds to say:]

In connection with the constitutional influence exerted by the

male, through the medium of the fœtus in utero, on the system of the female, another and a very singular question may be raised. In the case of an aboriginal woman of colour, does previous impregnation by an European male render the female incapable ever after of fruitful intercourse with a male of her own race?

This question is suggested by an observation, made in various parts of the world, by the excellent Count de Strzelecki, relative to the effect of fruitful intercourse between an aboriginal female and an European male. "*Whenever such intercourse takes place,*" says the Count, "*the native female is found to lose the power of conception on a renewal of intercourse with the male of her own race, retaining only that of procreating with the white men.*"

This, if a general fact, contrasts remarkably with Dr. Simpson's case, above mentioned (one of fruitful connection between a white man and a white woman, after previous impregnation of the latter by a black man), unless, indeed, this be, which probably it is not, an exception to an equally general fact of the same sort. It would limit, also,—nay, absolutely exclude, opportunities of observing whether children born of dark parents, where the mother formerly had issue by a European male, exhibit traces of the latter. But it was before stated, on the authority of two gentlemen long resident in Jamaica, that in our West India colonies—in Jamaica, at least—fruitful connections of this kind are of common occurrence, and (which I mention at present as in keeping with this) on the authority of Dr. Dyce, that, in children born under such circumstances, marks of the European have been observed. Special inquiry, made recently, have served so far to confirm these statements, but not to satisfy me that the issue of such connections is numerous.

The opportunities, however, enjoyed by the Count de Strzelecki, of making observations as to this point, in most parts of the new world, have been very great. "He has lived much (to use his own words) amongst different races of aborigines,—the natives of Canada, of the United States, of California, Mexico, the South American Republics, the Marquesas, Sandwich, and Society Islands, and those of New Zealand and Australia." And, referring to the statement made by him and already quoted, the Count observes—"*Hundred of instances* of this extraordinary fact are on record in the writer's memoranda, *all recurring invariably under the same circumstances*, amongst the Hurons, Seminoles, Red Indians, Yakies, (Sinaloa), Mendosa Indians, Araucos, South Sea Islanders, and natives of New Zealand, New South Wales, and Van Dieman's Land; and all tending to prove that the sterility of the female, which is relative only to one and not to another male, is not accidental, but follows laws as cogent, though as mysterious, as the rest of those connected with generation."

The alleged relative sterility of the native female, after intercourse with the European male, is brought forward (it may be added) by Strzelecki as affording an explanation, and as being the chief cause of the gradual diminution and ultimate extinction of

the native tribes in most parts of the new world, which follow the introduction of the European races. "Wherever the white man has set his foot-mark, there the print of the native foot is obliterated; and as the tender plant withers beneath his tread, so withers the aboriginal inhabitant of the soil." And "human interference," says Strzelecki, "to avert this melancholy consequence has been hitherto of no avail; a charter for colonization granted to one race becomes virtually the decree for the extinction of the other."

Monthly Journal, Oct. 1849, *p.* 1130.

128.—ON STERILITY.

By Dr. Oldham, Physician Accoucheur to Guy's Hospital.

[Dr. Oldham has many just objections to the meddlesome processes which have recently been adopted to remedy various displacements and functional or organic diseases of the womb. These he alludes to in the following observations:]

There are few cases which come before an obstetric practitioner which are so full of perplexity as those of sterility, especially where it is limited to those cases where the os and cervix, and body of the uterus, are free from any recognisable disease. Recent researches have afforded most valuable information on the composition of the male and female generative elements, and the physiology of generation; but our knowledge of the various causes by which impregnation is intercepted or prevented is very limited. One of these, no doubt, is any such partial or complete occlusion of the sexual canals as to prevent the transmission of the semen. Others are to be found in imperfectly developed ova within a shrunken ovary, or some defect in the semen, or a want of congruity between the two elements. These are subtle and concealed causes, difficult, and, with our present knowledge, almost impossible to detect, but of infinitely greater importance in their relation to primary sterility than the mechanical obstacles which have of late so exclusively engaged attention. It appears to me that the cases which justify the use of mechanical treatment require the greatest discrimination, not only on account of the facility with which they may be confounded with perfectly natural conditions, but also because these operations are not without danger. There is scarcely any amount of danger or pain that women will not go through to obtain the prospect of becoming mothers. They are notoriously credulous as to success, and are the ready, and often the costly victims of empiricism; and I would venture to say that obstetricians ought to be nicely scrupulous in encouraging a plan of treatment of very doubtful efficacy, and dangerous to life. I cannot imagine a position more overwhelmingly distressing to any right-minded man than to have been the means of destroying the

life of a woman in the endeavour to remove sterility. And yet I am sure that, in these operations, a hazard is run quite disproportioned to the amount of good accomplished; and I shall recount two fatal cases which have come to my knowledge: and I cannot but infer that others of a similar kind have occurred, but have not been recorded side by side with those of a more fortunate issue.

I feel great confidence in saying that a true congenital stricture of the os uteri, externum or internum, or of the Fallopian tubes, sufficient to prevent impregnation, is *very rarely* to be met with; and yet nothing is more easy, with the idea of a mechanical impediment in the mind, than to be self-persuaded into the belief that the natural orifice is too small. It is quite impossible to fix a standard size for the inlet to the womb. It has often happened to me to feel the virgin os uteri extremely small, and yet pregnancy to take place. The sound, too, is a very insecure guide to the measurement of the os internum; and I think it is a most reprehensible practice to allow a neuralgic dysmenorrhœa, whose seat I believe is generally in the ovary, to be the indication for this meddlesome practice. The only cases, in my opinion, in which a mechanical dysmenorrhœa with sterility can be said to exist, are those in which the tissue of the cervix is large and firm, and the os uteri is diminutive in comparison with the size of this body: a small, almost imperceptible, round aperture, perforating a bulky cervix. When the tissue of the cervix is not so condensed, but has its normal yielding feel, I doubt altogether the propriety of regarding even a very small os uteri as a strictured one. I have myself successfully treated by dilatation some cases of the kind above cited, but they are *very few*, compared with the large number which come under my care.

I. I am indebted to my friend Dr. Golding Bird for the following instructive case. On April 7th 1849, I received from him the uterus and appendages of a lady who had died from peritonitis, excited by attempts to cure sterility by mechanical dilatation, whose history, as furnished to me by Dr. Bird, is as follows, and with whose concurrence I publish it:—

"A lady of dark complexion, aged 36; married several years, and never pregnant; resided in Jamaica. From youth she suffered intense dysmenorrhœa, and always had pains during sexual intercourse. She was nervous, hysterical, and excitable to the last degree, and was supposed to have suffered from every possible form of inflammation; these attacks obviously being neuralgic, so common in hysterical women. In June last by the advice of her physician in Jamaica, she came to London for the express purpose of having the os uteri dilated, which had already been attempted by wax dilators. The obstetric physician who was consulted in London coincided in this opinion, and thought the sterility and dysmenorrhœa depended on a stricture of the os uteri. He divided the os uteri with a cutting instrument, and introduced silver dilators. This produced horrible suffering; and although at first she fancied the pains of menstruation were rather better, they soon became as

bad as ever, and she did not experience the slightest relief. She left off the treatment for a time, but was soon again inclined to resume it; and silver canulæ were passed into the os, and left there. Again she suffered frightfully. On Saturday, March 31st, a gentleman, the assistant of the physician, passed in another tube, but the distress was intolerable; and sickness and shivering coming on, she urgently begged her sister to try and remove it, which she succeeded in doing. Getting worse, a neighbouring surgeon was summoned, and he found her labouring under what he regarded as peritonitis masked by hysteria. She had scarcely any fever, collapse coming on almost immediately, and she continued sinking until Tuesday, when I (Dr. G. Bird) was summoned to her. I found her at her sister's residence at T——— Park, presenting almost the collapse of cholera; the pulse 200, and a mere thread; distended abdomen; vomiting of black fluid; intense irritability. All treatment was useless and she soon sunk. On examining the body, and raising the omentum, no appearance of disease of any kind was found above a line connecting the anterior superior spinous processes of the ilia. Below this line there was intense peritonitis; the convolutions of the intestines covered with butter-like lymph, and the pelvis filled with pus-like fluid; the right ovary and broad ligament covered with the same butter-like lymph, but so feebly adherent that it washed away by dipping it in water; the cavity of the uterus was filled with bloody mucus. There was no other disease."

The uterus and appendages examined by Dr. Oldham:—

The uterus had been opened by a single oblique division of the anterior wall, directed from the cervix to the left angle of the womb. The uterus was larger than usual for the virgin; it was rounded on its anterior surface, and a bulging convexity of the posterior wall, which, with the general softness of the tissue, showed it to have been the seat of recent engorgement.

The blood vessels over the entire surface of the uterus and appendages were injected with blood, especially the fimbriated extremity of the tubes, the ovaries, the broad and round ligaments. On the anterior surface of the body of the uterus were two small projecting fibrous tumours, the size of a large and small pea; the serous investment of them was highly vascular, the blood-vessels rising over them just like the calyx of the ovarian ovum of the bird. There was a similar more flattened growth in the posterior wall.

The divided surface of the anterior wall shewed its proper structure to be much enlarged (it measured in the body eight lines); the muscular structure was soft, and the veins large: a probe easily ran through them. The length of the united cavities was two inches and ten lines, the canal of the cervix being one inch five lines. The mucous membrane of the cavity of the body was soft, slightly raised, and of a vermilion hue. Agitation in the water was sufficient to loosen and separate it.

At the os uteri internum there was a zone of highly-injected blood-vessels, broken only at one point: the circumference of this aperture was eight lines. The os externum had a clean, smooth

edge, without any break or mark of division: its circumference measured one inch one line. The cervix had its characteristic markings, and the glands were empty of mucus. On the right side of the divided cervix, which would have formed the front wall, the ribbings were stretched upwards, enlarging the mesh-like appearance; and towards the os internum some were lacerated transversely, and from this to the os externum the structure was more ragged than usual.

The right tube.—The extremity of this tube was almost entirely closed as a congenital formation, the aperture being very small. When opened, the fimbriated end shewed its characteristic rich folds of mucous membrane, which were much injected, and were covered with bloody mucus. The remaining two-thirds of the tube was apparently healthy, not vascular, and pervious throughout.

The right ovary was almost covered with lymph, soft, and large. There was a cyst large enough to hold a small nut on the uterine end of the ovary. The stroma was gorged with blood. There was only one puckered Graafian follicle: the surface of the ovary was thick and corrugated.

The left ovary was irregular in its shape, a projecting mammillary portion coming out from its outer end. This, on being cut into, was hard and vascular, like the commencement of malignant disease: the ovarian tunic was thick and wrinkled; the stroma vascular: a few remains of Graafian vesicles, with puckered tunics, and some clots of different colours, black and brownish.

The left tube vascular at its fimbriæ, healthy in its mucous membrane, and its canal pervious throughout. This tube passed into the uterus more directly than its fellow, which was more curved. The veins healthy; arteries healthy; the right round ligament large and vascular; vagina healthy.

It is unnecessary to comment at any length upon this case. It affords a most instructive example of the dangerous effects of dilatation, even in experienced hands, and the great caution with which it should be undertaken. It is important, too, as shewing the difficulty of detecting the cause of sterility. I am sure that there was no kind of morbid contraction in this case, and that the os and cervix uteri, which were alone treated, had nothing whatever to do with the dysmenorrhœa or sterility. Both of these, no doubt, were dependent on the atrophy of the ovary; and the congenital obliteration of the end of the right tube would have been sufficient to exclude the corresponding ovary from any share in the function of reproduction.

II. Another presumed cause of sterility and dysmenorrhœa is, any deviation in the position of the uterus, and hence an indication for the cure of these disorders is to replace this organ, and hold it in its proper axis in the pelvis, by means of Dr. Simpson's uterine supporter. Dr. Rigby and others have related cases of this kind. It is not necessary for me to reiterate the objections which I urged in the last number of the Reports upon this subject, but I cannot avoid relating the following case, which more than confirms my

opinion of the dangers which may arise from this supporter. I am indebted to Mr. Bransby Cooper for this case, which, like the preceding one, ended fatally; and which he has given me his permission to publish.

A young married lady, of great personal attractions, was attended by Mr. Cooper, for a very painful fissure in the anus, which he divided and speedily cured. She then spoke to him of what had been to her a very distressing social trouble, namely, her sterility, which she associated with a perfect indifference to sexual intercourse. Mr. Cooper examined the sexual organs; but as he did not discover any defect which could be remedied by surgery, he referred her to a physician-accoucheur. This gentleman detected the uterus in a retroverted state, which he looked upon as the probable cause of the sterility. For the cure of this displacement, he introduced a uterine stem supporter, which set up peritonitis, of which she died in three days.

It is much to be lamented that the warning which such a case as this imperatively suggests should not have been published by the obstetric physician in whose practice it occurred. My own opinion is, that mere displacement forwards or backwards, if the uterus be not diseased, does not commonly cause sterility; and I cannot but characterize the practice of fixing the womb in a definite position by means of a stem supporter as rash and hazardous, causing severe irritation and pain, and even death to the patient, with, at the best, a very questionable amount of ultimate good. The anteversion or retroversion of a small uterus, without other complications, does not, in my experience, occasion any great distress; and it is far better to leave it alone, and improve its tissue with the rest of the organs of the body, than to prop it up for a time under the feeble pretence of curing it.

III. Dr. T. Smith's adventure of catheterizing the Fallopian tubes I know of only from his papers. I have the instrument by me, but at present I have no intention of using it.

It may, perhaps, appear presumptuous to express an opinion without some experience concerning it; but the grounds on which it is sought to establish it as a practicable and useful operation appear to me to be in great measure hypothetical and insufficient. The operation itself is attended, of course, with the same danger of local irritation as the uterine sound, aggravated by the time necessary to perform it. The canula and probe may no doubt be passed, as a demonstration, to their respective distances in the sexual organs when these are healthy and removed from the body; but I feel persuaded that the difficulties of accomplishing it in the living body cannot well be exaggerated. The obstructions to the tube are principally of a solid kind—deposits of scrofulous matter, the girthing of false membranes, &c.: these, of course, cannot be overcome, neither can they be detected. From what facts the presence of mucus in the Fallopian tubes, sufficiently firm and thick to block up their canal, is inferred, I do not know. It has happened to me to examine carefully these tubes in a great many inspections, but I

have never seen such a secretion. From what I have seen when the tube has been dilated, and its secretions retained, I should say that the mucus was thin and fluid, and very different from that of the cervix uteri, to which the term 'thick' may well be applied. Again, I doubt very much if any probing, such as is contemplated by Dr. Smith, would remove a thick mucus, supposing even the mucus was there, and the probe could be passed. The only way in which such mucus could be dislodged would be by injecting the tubes, which would be dangerous, on account of the fluid escaping into the peritoneal cavity. Indeed the operation appears to me to be altogether indefensible, and, but for the authority of so good a name, it could hardly be rescued from the charge of extravagance.

Guy's Hospital Reports, Vol. VI., Part 2, Oct. 1849, *p.* 371.

129.—*On the Use of Iodine in Congestion and Erosion of the Cervix Uteri.*—By Dr. Churchill, Dublin.—The preparation Dr. Churchill employs is of the following strength:—

℞. Iodinii ℥j.; potassæ hydriodatis ℥ij.; aquæ destillatæ ℥ij.; spiritus vini ℥ij.

and after four or five years' trial, he can truly say that he has found it the best application for congestion, erosion, or superficial ulceration of all that he has tried. Dr Churchill usually commences with a single application of nitric acid, or the acid nitrate of mercury, and then, after a few days, he paints the entire cervix with the iodine. This must be repeated once or twice a week, but not oftener, for whenever he has attempted its more frequent use he has found the uterine irritation rather to increase than diminish; and this will probably explain why these trifling complaints take so long a time to cure. Dr. Churchill has seldom succeeded in less than two months if the congestion was considerable, and many cases have required a much longer time. The application occasions no pain at all, unless the orifice of the vagina should be touched by the caustic, which may happen if it be applied too profusely. In one case the patient complained of a metallic taste in her mouth, in five minutes or less after each operation. After one or two applications, the cervix will generally be found to have diminished in volume, to have lost its tenderness, and the eroded surface to have lessened in extent, and to have assumed a more healthy appearance. Dr. Churchill strongly recommends that the application of the iodine should not cease abruptly, but first be diminished in frequency, then left off, and resumed if, as is very common, any of the symptoms return.—*Medical Times*, May 14, 1849.

Monthly Retrospect, July, 1849, *p.* 150.

130.—*On the Application of Leeches to the Os and Cervix Uteri.*—By W. Clegg, Esq., Boston.—[After remarking upon the difficulty there is in applying leeches to the os and cervix uteri, when the cervix is displaced through hypertrophy of the womb or relaxation of its ligaments, Mr. Clegg says:]

I have lately used a very simple instrument made for me by Mr. Wood, of York, which has answered admirably, and I therefore have much pleasure in describing its construction. An oval cup made of plated metal, very smooth externally, roughened internally, and about the size of a split walnut, so that it will easily pass through a speculum, contains the leeches. To this is attached a hollow stem, about nine inches long. The cup is covered by a lid, and to this lid a wire is attached which runs the whole length of the stem. The cup is passed through the speculum, and by reason of its extreme smoothness is rapidly slipped beneath the enlarged and prolapsed cervix so as to be closely applied to the os; then the wire is drawn, the lid slips back, and the leeches bite eagerly at the diseased surface. In one case, where the cervix dragged very heavily upon the rectum with considerable retroversion of the womb, I applied leeches *without* using the speculum, the cervix being too tender to allow of the least contact with that instrument. The leeches (four in number) laid hold warmly, and after filling themselves, which they did in half the usual time, found their way out of the vagina in the most orderly manner.

Medical Times, June 2, 1849, *p.* 604.

131.—*On a Case of Fibrous Tumour of the Uterus.*—By Dr. E. Ballard, East Retford.—[The history of this case was almost unknown to Dr. Ballard. At the post-mortem examination of the body he found the peritoneal cavity to contain a large quantity of fluid, and to present the evidences of old and of recent inflammatory action. Connected with the upper part of the uterus, and evidently formed in its fundus, was a tumour of the size of an adult's head, fibrous in its texture, and containing a cavity (quite distinct from the true uterine cavity), which communicated with the peritoneal sac, by a perforation through which the fluid had escaped into the peritoneum. The cervix uteri was found to have disappeared. Dr. Ballard makes the following observations on this case:]

1. Fibrous tumours of the uterus are by no means uncommonly found after death in women who are carried off by all kinds of diseases. If small, and placed beneath the peritoneal surface, they may be productive of no inconvenience whatever, but if large they may produce much local uneasiness; or by pressing on neighbouring organs, as the bladder or rectum, disorder their function, and impede more or less the evacuation of the urine or fæces. It is not common for them to prove fatal, but the above case exhibits one mode in which they may become so. I have already illustrated one change which fibrous tumours of the uterus undergo in process of time, namely, their condensation, and more or less perfect calcareous degeneration. This, also, is one of the changes to which tubercle is subject; but as this matter is capable of a process of softening, so, too, is the fibrous tumour. It was in this manner

that the cavity which I have described became formed. Whether originally due to inflammation or not, (and I should be disposed to regard the latter as most probable), that process was evidently going on at the time of the patient's death, as evidenced by the large quantity of lymph which was discovered in the cavity. The disease had proved fatal by the extension of the softening process to the external surface, and perforation of the peritoneum.

2. Directing our attention to the peritoneum, it is observable that there appeared to have been no endeavour made at preventing the effusion of the contents of the morbid cavity by the formation of adhesions. Where perforation of the wall of the stomach succeeds as the result of a large ulceration of the mucous surface, it is common for adhesions to occur to a neighbouring organ; but in the instance before us, the cause of perforation was probably not an inflammatory one, the opening through the membrane taking place by gangrene, from defective vascular supply, the substance below it undergoing death. Perhaps, also, there was present some amount of liquid effusion in the peritoneum previous to the perforation. The peritoneal membrane exhibited indications of recent and of former inflammatory action, the latter in the firmly organized coating of lymph which covered most of the parietal peritoneum, and the former in the ascitic effusion, vascular discoloration, and recent lymph. The one was probably the result of irritation of the large solid tumour, the latter clearly due to the perforation. The large amount of effused fluid may be accounted for by the congestive character of the inflammation. It was remarkable that there was no suspicion either of perforation of the peritoneum, or of peritonitis, excited in the mind of the medical attendant by the symptoms presented by the patient; no sudden occurrence of pain induced by the former, and no local pain and tenderness to indicate the latter. The age of the patient will readily account for this, since the obtuseness of sensibility of advanced years tends to render latent inflammatory disorders, which, in a younger person, would exhibit themselves by marked nervous phenomena.

3. I desire lastly to draw attention to the disappearance of the cervix uteri in connection with a tumour of the fundus of the organ. I have already noticed a similar occurrence where the body was greatly enlarged by being the seat of hypertrophy, fibrous tumour, and cancerous growth. In these two instances it was plainly due to the changes the organ had undergone; in others, however, the cervix is only partially destroyed, from adhesions occurring between it and the neighbouring part of the vaginal wall.

Provincial Medical and Surgical Journal, June 27, 1849, *p.* 345.

132.—*On Uterine Polypi.*—By Dr. LEVER, Physician-accoucheur to Guy's Hospital.—[In Guy's Hospital Reports, Vol. vi. Part 2, Dr. Lever has inserted several interesting cases of polypi which he fully describes. He then makes the following interesting remarks:]

Continually fretting the portion of the womb with which they come in contact, they produce central uneasiness, pains in the loins, groins, and thighs; give rise to a sub-acute or chronic inflammation of the neck of the uterus; enlargement of its special glands; sometimes hypertrophy, with induration; and in most instances superficial ulceration. The discharge is very like the decoction of quince-seed; and when ulceration is present, the glutinous discharge is usually mixed with one of a muco-purulent colour, and occasionally streaked with blood. Sometimes these growths co-exist with polypi of a fibro-cellular structure. Patients may have several of these polypi removed at different periods.

Both the married and the unmarried are, I believe, equally liable; and the patients not unfrequently continue for weeks or months under medical treatment, until tactile or visual examination discovers the *causa mali*. The treatment is simple—removal either by forceps or scissors; reduction of inflammation, if such be present, by local depletion; washing away discharge, and relief of pain by medicated injections; and the cure of superficial ulcerations by the occasional application of the arg. nit. These principles carried out will in every case, I believe, effect a cure.

I have frequently known polypus uteri lead to abortion. The age of the patient reported in one case was young, if we take into consideration the size of the growth, and its freedom from malignancy: although the tumour itself, at the first view, impressed one with the idea of a malignant growth, yet, on close inspection, this was found to be due to ulceration, extensive, and in some places deep, which had formed on several portions of its base and sides; and from some of these ulcerations pale but coarse granulations were spreading. In the operation for the removal of the tumour, in one case, the single tube was employed; in another, the double canula; to both were applied the rack at the handle, which so much facilitates the tightening of the ligature. When I can use the single tube I prefer it, as it is not so likely to injure the patient if she should make any sudden move during sleep; neither is there the same necessity to keep her on her side as when the double canula is employed. In all cases where the canula is employed for the removal of polypi the bladder should be closely watched, as I have known much pain and unnecessary fear occasioned by its distention being overlooked.

When these morbid growths are noosed, it is better to use the largest whipcord that can travel through the canula, and not to tie the tumour at first too tightly, for the structure of the mass is prone to lacerate, and copious bleeding might ensue. Further, it is better to lighten the ligature every other day, instead of daily, as is the practice in the treatment of firm and non-malignant polypi. Again, if there be much hemorrhage at the time of operation, it will be a wise precaution to use the tampon, confining this by a compress placed over the vulva, and keeping all in position by means of the T bandage. I need hardly add, that the bladder demands constant watching. I prefer the plan of removing these

tumours by ligature, if such can be accomplished, to the application of pure nitric acid or the use of the nitrate of silver. If the former be employed, due care must be taken not to touch the vagina, as fatal peritonitis has been known in some instances to ensue.

Guy's Hospital Reports, Vol. VI., October 1849, *p.* 209.

133.—CASE OF SUCCESSFUL EXTIRPATION OF THE UTERUS

By Dr. C. H. Higgins,

Corresponding Member of the Edinburgh Obstetric Society.

[Dr. Higgins's patient was a maid servant, 17 years of age, who was unmarried and had never been pregnant. The catamenia had appeared for the first time about six months previously, the discharge lasting three or four days. At the next period the discharge was very profuse, and she attended for about three months at the Taunton and Somerset Hospital, as an out patient, for menorrhagia. At the end of that time the sanguineous discharge ceased to be constant, but during its intervals she had a fœtid leucorrhœal discharge. The further progress of the case is thus stated:]

By her own account she continued in this improved condition, capable of attending to the duties of her situation, until about three weeks since. At this time, however, (early part of April, 1843), she experienced a most severe menstrual period, attended with very violent and continuous bearing down pains in the natural situation of the womb, and in the back; which, from her description, clearly resembled those that characterise the expulsive efforts of the uterus during the latter stage of labour. These, according to her, tremendous pains, persisted for some hours, and then suddenly ceased, and she immediately discovered a large substance filling up the vagina, and projecting beyond the labia. She suffered greatly from dragging pain, and in the course of a day or two a considerable fœtid sanguineous discharge came on, and she became too ill and depressed for her menial duties. She was now seen by the usual medical attendant of the family—under whose care she remained for some days, and at his suggestion, she was at length sent into the hospital, and placed under my care on the 20th of April, 1843.

I saw the patient, for the first time, on the day of her reception, and found her with a countenance expressive of great depression and suffering. The pulse very low, but quiet and regular. The usual excretions were said to be performed naturally, and her appetite was tolerably good. She chiefly complained of unceasing pain of a *dragging* character at the lower part of the abdomen and back, and consequent complete loss of rest. Upon examination, in the presence of two or three of my colleagues, there was found, occupying the vagina, and protruding beyond the external genital fissure, an irregularly shaped mass, about the size of an adult human heart, and consisting of two distinct, but connected portions. A firm

fleshy substance of pyramidal form, covered at its base by a nodular fungoid-looking insensible growth of a spongy consistence—somewhat resembling the cauliflower excrescence of Dr. Joseph Clarke.

The growth, here and there, presented a broken down and ulcerated appearance, giving out a copious fœtid and bloody discharge. Upon more closely investigating the nature of the connection which existed between these two apparently dissimilar portions of the tumour, a firm tough pedicle, half an inch in diameter, and somewhat less than this in length, was perceived to arise from the centre of the former body, and to lose itself in the other mass; of which mass the greater portion merely overlay its more solid neighbour, as if it had been flattened and pressed out upon it.

The opinion formed of the case at this stage of the examination, was that a double fibrous polypus had been generated in the cavity of the uterus, and extruded by a sort of slow labour, at a menstrual period, when the womb would be more disposed to take on such an excited action; and that the more distant half of the tumour had become decomposed and broken down by exposure to the air and friction. In order, however, to arrive at a more precise diagnosis of the case, I proposed to remove the looser and broken portion at once, as it was clear that the true nature of the ailment could only be determined by a more intimate examination of the solid part of the mass, which was at present obscured to a considerable extent by the other. It was easily lifted up and scraped away with the handle of a scalpel, until the pedicle was reached. This being of a firm fibrous structure, required to be divided. The whole being in this way got rid of, the now denuded fleshy body was seen to be an uniform reddish, pyramidal substance, somewhat flattened from before backwards, about the size of a turkey's egg, firm to the touch, very sensitive, and having its narrow end high up in the vagina. The surface was roughish, and bathed in a fœtid bloody secretion, with here and there a slight abrasion, within the area of which might be seen traces of fibres, resembling muscular fibre. Upon making a vaginal examination I discovered, instead of a dilated os and cervix uteri, as was anticipated, at about three or four inches up the passage, a complete *cul de sac*, and immediately below this, a prominent ridge or thickening, encircling the apex of the tumour. These unlooked for signs, taken in conjunction with the roughness of the surface, presenting so marked a contrast to the usually smooth and almost polished appearance of a polypus, and the absence of every thing like a uterine mass above the pubis, together with the history of the case, satisfied me that we had to deal with something more serious than was at first conjectured. That, however unlikely *a priori*, the case was in fact one of those rare instances in which a polypus connected by a short pedicle with the internal fundus of the uterus had been suddenly expelled, and, dragging down with itself the organ, had caused it to undergo complete inversion; and that the broken-down mass, which I had removed in the early part of my examination, was indeed the polypus—the original cause of all this mischief; and, which as far as one could judge from its present

condition, was of about the same size as the displaced uterus. Some doubts, however, of the correctness of my opinion being expressed by those around me, from the improbability of such an occurrence, as inversion from polypus in a person of the youth and character of my patient, she being a virgin, as well as from the extreme difficulty of accurately distinguishing between inversion and polypus, I proposed to search for the originally *internal* orifices of the Fallopian tubes, which, if my judgment of the case were correct, should be found on the surface of the tumour. These I was fortunate enough to hit upon, and having inserted into each of them a common bristle to the depth of two or three inches, I succeeded in gaining over my less hasty colleagues to my own view of the case.

During these various and long-continued manipulations, from which the patient appeared to suffer very little, about six or seven ounces of blood were lost, but chiefly during the removal of the polypoid mass.

[For some time afterwards, the patient was kept in the recumbent posture, and appropriate local and constitutional treatment adopted; but the exposed fundus uteri assumed an inflamed and excoriated condition, and the patient's health began to fail so rapidly that it was determined, as the only resource, to extirpate the womb. Dr. Higgins thus describes the operation:]

The patient, having been previously prepared by a small dose of oil over night, and an enema early on the following morning, was brought into the theatre on the 5th of May, 1843, and placed in the position adopted for lithotomy (but without being tied). The labia being held aside by two of my colleagues, and traction downwards and outwards made on the uterus, I passed, by means of a double canula, or rather a modification of this instrument, the two parts of which could be separated or brought together at will, a tape band half an inch wide, as high up on the inverted portion of the vagina as possible, both as a precaution against hemorrhage, and also to give me a command over the divided vagina afterwards; and having tightened this band somewhat, I excised, with a common scalpel, guarded to within an inch of its extremity, the entire uterus, close to the tape band, and through the portion of the vagina immediately beyond the cervix. The excision was easily accomplished, and could not have occupied many seconds, and there was scarcely any hemorrhage. A double-bladed speculum being now introduced into the vagina, I passed three silk sutures through the entire substance of this part, at some little distance from its cut edge, on two opposite sides, and brought it into accurate apposition, as I should have done in an ordinary excised wound, so as to procure, if possible, adhesion by "the first intention," an anticipation in which I was not ultimately disappointed. I now ventured to slacken the tape band slightly, but a flow of blood instantly taking place, I retightened it, and at once arrested it. Having satisfied myself that the band was not applied so tightly as to endanger sloughing, in fact,

that it was acting simply as a tourniquet, I left it there, fastening the projecting end of the double canula against one of the thighs; and having filled up the vagina with a sponge moistened in cold water, the patient was replaced in bed. She bore the operation well, but subsequently complained sadly of the pain and distress it had cost her. When visited soon after the operation she was extremely depressed, and appeared to suffer greatly from abdominal and lumbar pains—for which she was ordered a full dose of laudanum in some wine, and perfect quiet enjoined.

[The subsequent treatment of the case it is unnecessary here to detail. The following is the report given by Dr. Higgins, for]

May 11th.—Much improved in all respects. The appetite becoming natural. From this date the patient continued steadily, and without drawback, to gain strength; and at the end of two months left the hospital, quite restored to health, and free from discharge. The vagina appeared about three inches long, and was closed by a complete cul de sac—a small ridge or line of cicatrix running through its middle. The wound of the vagina had completely healed at the end of a week.

I have had frequent opportunities of seeing and examining my poor patient since her removal from the hospital. On the 19th of February 1844, she came to see me, having walked a distance of three miles in about an hour; she was not at all fatigued, and she intended walking back again. She stated that she was, and had been, quite well, and should not, from her own feelings, know that anything unusual had occurred to her, except that she did not menstruate. She was very cheerful, and grateful for her recovery. On the 29th of July I again saw her; she was looking exceedingly well and cheerful. She told me she had never experienced the slightest discomfort, except periodical pains in the loins once a month, which lasted two or three days; but she had never had any sort of discharge, which could be regarded as vicarious menstruation. I have frequently seen her during the last three years. The last time I did so was at a friend's house, where she was living in the capacity of nurse-maid. This was in May or June 1848, five years from the date of the operation; and she was quite well, and had never ailed in the slightest degree. She stated that she had not experienced the periodical lumbar pains so often of late.

[In commenting upon this singular case, Dr. Higgins first refers to the extreme youth of the patient, who is the youngest person affected with polypus uteri, of whom he has found any record. Next, speaking of inversion of the womb as a consequence of polypus, Dr. H. refers to several cases of partial inversion. He then proceeds to say]

Of the *complete* inversion of the womb from polypus, in addition to the instance which fell under my own notice, I have met with the record of three cases—one given by Von Sauden, and quoted at length by Dr. Crosse; a second, given by the late Dr. D. D.

Davis, in his "Principles of Obstetric Medicine," Vol. I.; and a third, by Dr. Fleetwood Churchill, in his admirable work "On the Principal Diseases of Females," second edition, page 317, which he saw in the Jervis Street Hospital, Dublin, under the joint care of Dr. Montgomery and Mr. Lynch. Dr. Churchill refers to a similar case, related by Dr. Browne, in the "Dublin Journal;" but as I have not the opportunity of consulting this periodical, I must content myself with referring to it on Dr. C.'s authority. I believe, that a due comparison of these cases with mine, will show that mine was not the least remarkable example of the malady, either as to its history, or the subsequent consequences to the subject of it.

The diagnosis of inversio uteri has been pronounced a difficult matter, where it is partial or slight in degree, and even when complete, it is liable to be confounded with polypus. I would venture to suggest, that the signs usually given as diagnostic of inversion, are incomplete and unsatisfactory without the plan that I adopted, of searching for the orifices of the Fallopian tubes, and inserting bristles into them, a proceeding to which I was led in consequence of doubts being expressed as to the nature of my case, after I had ascertained the existence of all the usual marks of the accident. Where it is successful, the proceeding I suggest must unquestionably remove all doubts on the subject. At the same time, I would caution the reader from regarding a want of success in his search for the Fallopian orifice as a negative proof of the non-existence of inversion; but I am disposed to regard the proceeding as not very difficult, or requiring any great amount of dexterity.

It has been questioned by some, whether extirpation of the womb is a justifiable proceeding; but, I believe, the objectors to this, no doubt, most serious operation, have been induced to pronounce their veto, from an almost exclusive examination of the proceeding in connection with malignant degeneration of the organ. In this class of cases, it doubtless appears a questionable undertaking, not only from the almost invariable want of success which has been recorded, but also from the growing impression in the profession, that the tendency to malignant disease, when once it has been pronounced, is rarely to be eradicated from the system. When the subject of extirpation of the womb for inversion is examined *apart* from such malignant fault in the organ or the system, there appears to me no reasonable objection to the operation, formidable as it assuredly is. The numerous instances of the successful issue of such cases, to be found in various treatises and journals, justify this opinion; and, as Dr. Churchill says, (op. cit., page 332)—"There is abundance of evidence to show that life may be preserved after the loss of the womb." "Rousset relates a case, where the uterus was destroyed by gangrene, and the patient recovered; and Roussett, Primrose, Radford, and Cooke, have given cases in which the uterus appears to have sloughed off without compromising the patient's life."

Having decided upon the propriety of the operation, it was a ques-

tion with me whether I should adopt the ordinary plan of removal by ligature, or deviate so far as to employ excision of the organ previously strangulated by a silk ligature, as recommended by Dr. Davis and Mr. Windsor, for at first I did not contemplate any deviation from the prescribed methods. But reflecting on the probable sources of danger in the operation of extirpation, it appeared to me that *hemorrhage* was the only one to be really apprehended, and I began to debate how far this might be prevented without subjecting my patient, already worn down by pain and discharge, to all the usual distressing and depressing symptoms attendant upon the application of a ligature, with the apparition of peritonitis in the back-ground. Believing that I could accomplish this object by means of a broad tape band in lieu of the ordinary ligature, so applied as to act as a mere tourniquet, and which could be readily slackened, or even altogether withdrawn, when the risk of hemorrhage had ceased, and which, from its flattened surface and width, would not lead to the strangulation of the part around which it was placed, I was induced to depart, in the way already detailed, from the operative methods hitherto employed. In addition to the comparative immunity from pain and constitutional fever, in my case, I would suggest whether the more rapid and satisfactory recovery of the patient were not in a great degree promoted by my tourniquet ligature keeping the divided parts generally not only in close apposition, but in bringing and maintaining similar textures on the opposite sides of the wound against each other, and by permitting the employment of sutures to preserve their close apposition undisturbed. How far I was justified in my reasoning, or in subjecting my patient to a proceeding unsanctioned by precedent, or in my hope of inducing union of the divided vagina by the first intention, by means of sutures, I must leave to the decision of others, earnestly but humbly suggesting a candid consideration of my operation, and a fair comparison of the *progress* of my case, *after the extirpation*, with that of those in which removal of the womb has been accomplished by means of a strangulating ligature.

Monthly Journal, July 1849, *p.* 889.

134.—*On the Use of Oxide of Silver in Menorrhagia.*—By Dr. J. J. Thweatt.—[Dr. Thweatt states that over certain forms of menorrhagia, oxide of silver exerts the same power that quinine exerts over intermittent, or mercury over syphilis. He says:]

The oxide of silver is best adapted to those forms of menorrhagia which depend on an undue excitation of the uterine organs, unaccompanied with high inflammatory action. Cases often present themselves where profuse hemorrhage makes its appearance at the usual menstrual period, or immediately after it has passed; in these cases there is an extraordinary excitation of the nervous system. The oxide of silver here often acts like a charm: calms the perturbation of the nervous system, and arrests the hemorrhage by its

astringent qualities. It should be given in large doses, and repeated at short intervals until some effect is apparent. Women after parturition are frequently troubled with a sanguineous discharge, distinct from the lochia, which it is difficult to remove by the usual remedies. The oxide of silver is an infallible remedy for this pathological condition.

There is one form of menorrhagia often met with, which often baffles the skill and experience of the ablest practitioners to remove; and I know of no form of disease which tries more the patience of both patient and physician. A remedy, therefore, in which any reliance can be put, is certainly a desideratum. I refer to the too frequent occurrence of the menses. The quantity of the discharge is sometimes larger and sometimes smaller than it naturally should be; the intervals are short, and, in many instances, the patients are never entirely free from some discharge; exhaustion and debility are the usual accompaniments of this morbid condition; impoverishment of the blood followed by a cachectic condition of the general system; the nervous system is deeply involved; palpitations of the heart become a great annoyance. There is likewise a general depression of the moral faculties; the digestive organs are ultimately implicated, and there is dyspepsia in its multifarious forms,—gastralgia, pyrosis, &c., &c. Spinal irritation is of frequent occurrence; the patient complains during the short intervals (when there are any) of dull pains in the pelvic region, with the sensation of weight or a bearing down motion in the uterus.

The oxide of silver is the only remedy in which any confidence can be placed to remove these symptoms. I have, under these circumstances, employed in vain the various preparations of iron and lead, together with the mineral acids: they afforded only temporary relief; but in every case in which I prescribed the oxide of silver, its action has been satisfactory.

I am aware that it is the opinion of those medical gentlemen who have experimented with this medicine, that it is inferior to the preparations of iron in those cases where the hemorrhagic tendency depends upon a general anemic condition of the system. This is the opinion of Dr. Lane; it is with all due deference to such high authority, that I express a contrary opinion. In this form of menorrhagia the true indication to be met, is the arrest of the abnormal secretion; this drainage of the general system. If we can accomplish this (under all circumstances) desirable object, we shall have paved the way for the use of the ferruginous preparations, and the carrying out beneficially the proper hygienic regulations for the restoration of the health of our patient.

My experience with the oxide of silver induces me to believe that its main action is upon the capillary circulation, and particularly upon the uterine capillary system; that its powers are specifically directed to the uterine system. Its operation upon the nervous system is that of a mild and unirritating tonic.

It is almost superfluous to state that this medicine is entirely nugatory, if not prejudicial, in those cases of menorrhagia which

depend upon organic lesions. It never should be prescribed in cases of high inflammatory action; until after the subjugation of the inflammatory symptoms by antiphlogistic means, when it may be administered with great benefit.

I am in the habit of prescribing this medicine in larger doses than usual. The only pathological effects I have witnessed from its use, in two grain doses, two or three times a day, were uneasiness in the lower bowels, sometimes attended with slight tormina and tenesmus. The symptoms were easily removed by an anodyne enema. They often, however, require no attention on the part of the physician. The usual dose, when intended to be continued for any length of time, is half a grain to one grain, twice or three times a day, according to circumstances: it should always be combined with a small quantitiy of opium or morphia. The oxide of silver blackens the stools.

American Journal of Medical Sciences, July, 1849, *p.* 69.

135.—*New Remedy for Leucorrhœa.*—In the *Boston Medical and Surgical Journal*, Dr. BRAMAN states that for nine years, he has been in the habit of using preparations of the helonias dioeca (unicorn plant) as a remedial agent. He considers it particularly applicable in such affections as have their origin in atony of the generative organs of both sexes,—but particularly the female. "In leucorrhœa," he says, "I consider it invaluable. I use it with a confidence I *attach to no other medicine.* Under its influence the patient, whose life has been almost a burden, soon revives. Her uncomfortable sensations vanish, and, ultimately, an entire recovery of health and strength is established." It may be given in the form of powder, tincture, or syrup; but the latter is the most eligible. The doses which Dr. Braman recommends, are, of the powder, one drachm and a half; of the tincture, one fluid drachm; of the syrup, three fluid drachms. These doses are to be taken thrice a day, half an hour before the ordinary meals; and, according to the urgency of the case, the quantity administered may be increased, if the patient bear it well. In irritable stomachs, nausea is sometimes produced; and when this occurs, the dose must be diminished.

[No formula is given for the preparation of the syrup or tincture.]

London Journal, Oct. 1849, *p* 962.

136.—*On Pelvic Cellulitis.*—By Dr. LEVER, Physician-accoucheur to Guy's Hospital.—[After detailing several interesting cases of this description, which are too often overlooked by practitioners, Dr. Lever remarks:]

Pelvic cellulitis, in the majority of instances, is not due to any mismanagement or neglect on the part of the puerperal attendant. It may follow abortion, and labour, either premature or at full

term. It will be found to come on after preternatural, complex, or flooding delivery; it sometimes arises after labours terminated artificially, whether by the hand or by instruments; and sometimes it is very prevalent during an epidemic puerperal fever. The child born may be small or large; but where there is a want of proportion between the child's head and the pelvis, and where such labours are lingering, or are terminated artificially, multiplied observations have convinced me that that part of the pelvis affected will be found to correspond with the position of the fœtal occiput. Patients of the strumous are more susceptible than those of a contrary diathesis. The symptoms usually supervene on the second or third day after delivery: in some instances they come on actively, in other cases insidiously; quickness of pulse and anxiety of countenance being the most constant attendants. In all cases the secretion of milk is small; the lochia diminished: in some the one is absent, the other arrested. Pain, moreover, is a constant symptom, at first confined to the part inflamed; but as the disease advances, and the inflammatory products are effused, the pain is increased, and the patient will point out with anatomical accuracy the course of the nerves upon which the trespass is committed. In some cases the disease is very masked, the patient even going out at the end of the month; but still, if strict inquiry be made, it will be found that she has not felt so well as after previous confinements, and that on one side or the other there have been pain and a want of power in the lower extremity. My experience leads me to believe that the left side of the pelvis is much more frequently affected than the right; and it will be conceded that the occiput of the fœtus is more frequently directed to that side. The decubitus of these patients is most marked; they lie on the back or on the sound side, with the thighs flexed, with the affected extremity resting for support on the sound limb and rotated inwards, giving the observer the idea of diseased hip-joint; this is most markedly shewn in the cases above related. The disease terminates either in resolution, suppuration, or absorption of the inflammatory deposit; the former is rarely seen in hospital patients, the second is not uncommon. The abscess may present or open externally, or it may burst internally. Where pus was discharged into the vagina, it will sometimes open into neighbouring viscera. The matter in some cases is found to burrow deep into the pelvis, and the bone may become diseased. But after suppuration has taken place, and the pus has become evacuated, and the abscess has closed, there will be considerable induration, in the removal of which, time and improvement of health are the two great remedies. Induration is perhaps by far the most common result of pelvic cellulitis.

These cases, when admitted into the hospital, rarely demand or admit any active treatment. I have never seen one where general depletion was called for or needed. Local depletion, repeated from time to time, according to the activity of the disease and the strength of the patient's constitution; warm poultices, simple or medicated; tepid anodyne injections carefully thrown into the

vagina; attention to the state of the bowels, which are prone to irregularity, at one time constipated, at another relaxed; the use of mild mercurials, until the gums become slightly affected, and maintaining this with gentleness, are in my belief the best means of treating the disease. At the same time the diet must be bland and nutritious, and the powers of the patient must be supported. When it is evident that suppuration has taken place, I am an advocate for evacuating the pus, so soon as it can be done with certainty and safety. The diet must be improved; porter, wine, &c., given with a free but prudent hand; while bark, quinine, and the mineral acids will aid in the recovery of the patient. To promote the absorption of the induration which follows suppuration, or to quicken the external progress of the pus, I place great faith in the employment of blisters. Absorption is still further promoted by pot. iodid., administered in some bitter infusion or in sarsaparilla.

Guy's Hospital Reports, Vol. VI., Oct. 1849, *p.* 219.

137.—*On the Treatment of Pelvic Abscess.*—By Dr. E. J. Tilt, Physician to the Farringdon General Dispensary.—[In speaking of the treatment of pelvic abscess, arising from ovarian inflammation or other causes, Dr. Tilt speaks first of incision through the vagina, which he considers to be in many cases a safe and proper mode of evacuating the abscess. Proceeding then to speak of opening the abscess by the skin, he says]

If fluctuation is not perceived in the vagina or in the rectum, but, on the contrary, is found in the hypogastric region, then the aperture must be made in that part of the abdomen towards which the tumour points. It would be highly imprudent to open the abscess without having effected an adhesion between the cyst and the abdominal walls, as we can never be sure that such has taken place.

Several plans may be adopted for this purpose, all borrowed from the treatment successfully employed in the cure of abscess of the liver; but that most generally made use of was first carried into effect by Professor Recamier, and consists in determining the adhesion of the two peritonæal surfaces by the application of Vienna paste, (potassa fusa cum calce,) previously to puncturing the cyst through the eschar.

The treatment successfully made use of by Drs. Graves and Begin, in abscesses of the liver, might also be adopted in this instance. Dr. Graves makes an incision of a portion only of the thickness of the abdominal parietes, and then applies linseed-meal poultices over the incision, and the pus almost always finds an exit where the walls of the tumours have been so divided. When an opening is once formed, it is important that the free issue of the matter be maintained. As this plan has been so successful in the cases alluded to, why should it not be equally so in those pelvic abscesses which point towards the skin? We know not, however, if this plan has received the sanction of experience.

Dr. Begin's mode of treating abscesses of the liver is similar to the preceding, inasmuch as he cuts down on the tumour until he reaches the peritonæum, but without dividing it. He then dresses the wound; and a few days after, when, as the result of inflammation, the parietal peritonæum became adherent to that portion of the membrane which covers the abscess, he punctures it, and thus gives issue to the pus. This plan of treatment might also be advantageously employed; but we have seen most of that adopted by Professor Recamier, and we generally make use of it.

Having decided in what part of the abdomen it is most desirable to effect an opening, a certain quantity of Vienna paste (potassa fusa cum calce, made into a paste with alcohol,) is applied to the skin; and when the thickness of the parietes requires more than one application of the caustic, it is better only to remove the central portion of the eschar, leaving the outward portion to protect the cuticle from the action of the caustic. When the seat of fluctuation is nearly reached by the caustic, and when adhesions have evidently taken place, from the impossibility of the abdominal parietes sliding over the tumour, then an incision is made in the centre of the eschar. Injections of tepid water should be made into the abscess, to remove fœtid secretions, and to impede the ingress of air, by keeping the abscess full of liquid.

Lancet, June 16, 1849, *p.* 636.

138.—*On Uterine Disturbance during the Climacteric Period.*—By G. Corfe, Esq., M.D., Middlesex Hospital.—There is no organ in the female economy that so readily partakes of disturbance from uterine irregularities as the liver; and the instances are not few where protracted menorrhagia has resisted astringents, quinine, and the ordinary treatment, whilst it has yielded to brisk and frequent purgation, with small intermediate doses of calomel, &c. In such cases the uterus takes on the action of the liver, and so long as the latter viscus is allowed to remain in its torpid condition, so long also will the uterus continue to pour out an undue quantity, or an altered quality of dark venous fluid, and the patient becomes enfeebled, harassed, and distressed; hæmorrhoids are not unfrequently induced also. But no sooner, oft times, do the remedies tell upon the hepatic branches of the portal system of the liver than the intestinal portion of the same system becomes relieved from its congested state, and the uterine discharge is shortly subdued.

It is from such observations that my mind was originally led out into the study of some painful attacks of the nervous system, connected with the climacteric period, and I can confidently assert, that a very large number of women, who would have been treated ten years ago with frequent bloodletting, and other antiphlogistic remedies, have been perfectly convalescent from a totally opposite method. The stomach, liver, and kidneys, appear to be the principal organs which suffer from derangement, and, in order to restore to the former viscus its healthy tone and vigour, scrupulous atten-

tion must be paid to the diet. Veal, pork, salt meats, fried and broiled fish, vegetables, except potatoes, tea, pastry, and made dishes, or ragouts, wine, bottled beer, or any liquor containing a free acid, or gaseous matter, appear to aggravate the disorder in a marked degree. Mutton roast and boiled, fish, poultry, fresh game, cocoa nuts boiled, or coffee, not too strong, and draught porter, is the most agreeable diet for such a stomach; but the benefit derived from taking six or twelve fresh oysters without any other food, or additions, as pepper, vinegar, &c., or even bread, upon an empty stomach, as for luncheon, for instance, has been most remarkable in some severe cases which I have witnessed. Their alkalescent character has removed the morbid acidity of the stomach, whilst it has restored its tone and vigour, and the following draught has been ordered to be taken every morning on rising:—

℞. Ammoniæ hydrochloratis grs. x.; extr. taraxaci, ʒss.; dec. aloes comp.; mist. gent. comp. aa, ʒv.; sodæ pot. tartratis ʒi.; tr. lavandulæ comp. ♏xx. Fiat haustus.

If this aperient should not prove of sufficient activity, it must be repeated in the middle of the day; but, on the other hand, if it is too active, the draught may be only taken every other or every third morning, yet it is of greater importance that the alimentary canal should be freely acted upon once or more daily, than that it should be allowed to continue in a torpid condition.

The alvine evacuations are usually offensive, fetid, and so dark, that I have known them to partake of the character of the motions of a patient with melœna for several days together, and I am inclined to think that it is from the fact that practitioners do so rarely submit these excretions to ocular examination, that many cases of this disorder are misunderstood, and therefore mismanaged.

Another useful remedy is to clothe the loins with the emplastrum opii, (Ph. Lond.) or else with a long strip of new flannel; and, in order to assist the liver in its healthy functions, a warm bath and occasional friction over the skin with sand-soap or a horse-hair glove usually promotes the healthy action of the abdominal viscera.

Whenever the urine ceases to deposit lithates, and becomes a pale canary colour, and the evacuations from the bowels present a healthy appearance, and cease to emit a penetrating fetor, there is a co-existing amelioration in the sufferings of the patient. Her spirits are more buoyant, her mind less distressed, her appetite more even, and her whole frame less weary.

Medical Times, April 7, 1849, *p.* 451.

139.—*On the Climacteric Disease in Women.*—By Dr. W. Tyler Smith.—[Dr. Smith remarks that the climacteric disease which occurs in women at the decline of the catamenia, is to be treated with reference 1st, to the cerebral symptoms; 2nd, to the ovario-uterine disorder; and 3rd, to the general hyperæsthesia. The principal point in the treatment of *the cerebral symptoms*, or rather in the prevention of the seizures, consists in the avoidance of all excite-

ment of mind or body, and the removal of all sources of irritation. In very rare cases, when there is much cerebral congestion, leeches or cupping may be required. As to the *ovario-uterine disorder*, Dr. Smith remarks:]

The treatment of the ovaria and uterus is the most important point, in the management of the female climacteric. The cerebral symptoms are only the *results*, while the conditions of the sexual organs are the *causes* of the more remote maladies. Any disorder of the uterus or ovaria, beyond that actually incident to the catamenial change, should be carefully treated. Irritation or abrasion of the os uteri, or displacement of the womb, or congestive enlargement of the organ, increases the severity of the constitutional symptoms. The most natural and effective remedy for all the uterine disturbances of this period, is moderate depletion from the *labia uteri*. The blood may be drawn by incisions into the os uteri, just as in scarification of the gums. I have often used a gum lancet with great good effect. Or if the depletion is required to be more considerable, three or four leeches should be applied, by the aid of the speculum to the os uteri. Leeches draw blood more suddenly from the vascular os uteri than from any other part of the body to which they are commonly applied, and often produce prompt and immense relief,—all the relief, in fact, of the catamenial secretion, without any of its inconveniences. The suddenness with which leeches applied to this part fill themselves, considerably increases the good effects of their application, and for some hours after their removal there is an oozing of blood from the leech-bites. Even in cases where hemorrhagic symptoms accompany the change of life, the application of leeches before the coming on of each uterine flow diminishes the menorrhagia, by diminishing the uterine congestion. Occasional local depletion, besides its importance to the climacteric disorder, tends more than anything else which can be devised, to prevent the structural diseases which frequently set in or exacerbate at this epoch. The efforts of uterine depletion in mitigating the most distressing head symptoms, is often almost marvellous. This form of depletion ought to be resorted to from time to time, during the change of life, according to the extent of the uterine congestion, in all women who suffer materially from this alteration of constitution. We often see indications of the treatment which is required in sanguineous effusion from vaginal or rectal varices, or from hemorrhoidal tumours, or even from the stomach and lungs. It is of course still better if the depletion be made from the uterus itself. The effect of uterine depletion is not confined to the uterus, but extends to the other parts of the sexual system, the Fallopian tubes, the ovaria, and even the mammæ. Occasional uterine depletion appears to me to be of the first consequence, not only as a method of cure, but as a means of preventing organic disease of the uterine organs, and of those organs affected by the uterine synergies. Many of these disorders, occurring at the change of life, appear to be aberrations of the formative power which, during the whole reproductive era, has expended itself upon ovulation, gesta-

tion, and lactation; but which, being now turned from these physiological processes, takes a pathological direction, and runs into disease. Thus then local depletion is of still more importance as a palliative and preventive of uterine and other diseases of this period, than as a means of removing more remote symptoms. In treating diseases of the reproductive organs, the special reproductive power inherent in these organs, and manifested even in their diseases, should never be lost sight of.

The local application of cold is an important auxiliary to depletion, at the female climacteric. Cold hip baths, or douche baths to the loins, cold water injections into the rectum, the injection of cold water, or iced water, or the introduction of small pieces of ice, into the vagina, are the modes in which cold can best be applied. Injections of any other kind than of simple or iced water, generally produce irritation at this period; and sometimes the vagina is so irritable, that even cold water will not be borne. In cases where occasional returns of the catamenia assume a hemorrhagic or dysmenorrhagic form, warm and anodyne injections, both into the rectum and vagina, are useful, acting as fomentations. Warm water and laudanum, or the infusion of poppy heads with laudanum added, allay uterine pain and excitement. When menorrhagia with relaxation is the prevailing symptom, strong alum baths,* in the intervals between the menstrual discharges, tend to repress the profuse uterine secretion, and to remove the relaxation of the uterine and vaginal tissues.

Care must be taken to avoid rectal irritation, as any excitement of the lower bowel is sure to be participated in by the uterus. The bowels should be kept in a lax state by cooling aperients, or enemata; but drastics, and particularly aloes, should be avoided. The habitual use of aloes, either as a purgative or dinner pill, has seemed to me to increase the uterine disturbance of the change of life, when this period arrives. The management of the bowels at this time is often a difficult matter, as the lower bowel participates in the uterine habit, and is at one time irritable and at another confined. To give aloes as an aperient at this period is mischievous; it is still worse to give it as an emmenagogue. All emmenagogue remedies are as distinctly contra-indicated at this epoch as during pregnancy. They are certain to produce mischief. But although the uterine secretion is not to be stimulated, the other important secretions—hepatic, renal, alvine, and cutaneous,—ought to be carefully regulated and kept in full play, to compensate for the important secretion which is about to become extinguished.

The General Hyperæsthesia.—The hyperæsthesia and the peculiar æsthetic paroxysms, to the study of which it has been my main object to draw attention, without doubt depend upon the erethism, or excitability of the nervous system, induced by the irritable condition of the uterine organs, and upon the partial suppres-

* I use the formula recommended by Dr. Ashwell, viz. $\frac{\text{ʒ}}{\text{ʒ}}$ xvi. of alum to each gallon of water; the temperature to be at 98 degrees.

sion of the catamenial secretion. All the treatment which has been mentioned contributes to the relief of the hyperæsthetic symptoms. I would, however, insist upon the importance of regulating the diet of persons undergoing the catamenial change. From the tendency to plethora and *embonpoint* at this period, a light, nutritious, but not full, diet, with little wine, and no malt liquors, should be prescribed. Owing to the distressing sensations common to this time, small quantities of spirits are sometimes ordered, and are always gladly taken by patients. There is often a diseased craving for stimulants at this time, which, in several instances, I have seen pass into a decided habit of spirit drinking. Spirits ought only to be allowed with the greatest caution at this time of the constitution, as their good is only temporary, while their permanent influence is most mischievous. I know of nothing equal to moderate doses of sulphuric ether and valerian, for the relief of the distressing hyperæsthesia, and also of the paroxysms, when these are slight. They often act like a charm in soothing the sensations of the surface of the body; and, given as medicine, they beget no such habit as the permission to take small quantities of spirits frequently does. In the climacteric state of the female constitution, sulphuric ether is a more decided sedative than either morphia, the preparations of opium, or even hyoscyamus. In the height of the hyperæsthesia, an ether draught will often procure sleep when the brain refuses to be soothed by narcotics. Throughout the whole period of the change great attention should be paid to the skin. The clothing should be carefully attended to, and, during winter, flannel jackets and drawers should be worn. Tepid bathing, with subsequent friction, are of great use in diminishing the excessive sensibility of the skin.

London Journal, July 1849, *p.* 601.

140.—*Case of Ovarian Disease.*—By Dr. G. Kennion, Harrogate. —[The patient, a lady twenty-one years old, had received a kick in the right iliac region, between four and five years previously, a few months after which a tumour appeared in that situation. Leeches were applied occasionally, iodine ointment rubbed upon the part, and hydriodate of potash given internally. Under this treatment the tumour was apparently removed, and she became quite well, except that the catamenia were suppressed. She continued in this state for nearly four years, when the catamenia reappeared (scantily) three times at regular intervals, and she then perceived a return of the swelling. It rapidly increased in size, and produced such oppression of the breathing, that tapping was resorted to. Only three pints of fluid, however, came away, and she died in a few days afterwards. Dr. Kennion thus describes the post-mortem appearances:]

On opening the abdomen a few ounces of straw-coloured fluid escaped from the peritoneal sac. The whole abdominal cavity was filled with an enormous congeries of cysts, (hydatid?) the coats of

which were so exceedingly thin that they could scarcely be touched without breaking. The size of these cysts varied from that of a filbert to the size of an orange; the fluid which they contained varied in colour and consistence, the larger number containing a similar fluid to that which had been removed by paracentesis.

The effect of this rapidly-increasing disease had been to push upwards the whole of the abdominal viscera to such an extent, that the caput cœcum coli, doubled upon the ascending colon, occupied the right hypochondriac region; the upper margin of the lesion was as high as the third rib, and the lower margin not lower than the fifth rib; while upon the left side, the stomach immensely distended with flatus, encroached still more upon the cavity of the chest. On removing as well as we could the diseased mass, it was found to be connected entirely with the right ovary and broad ligament, those parts on the left side being perfectly healthy. The right broad ligament was studded with growths of a very firm cerebriform character and appearance, from the size of a pea to that of a walnut; some of these were very highly injected, others were pale, and contained an embryo cyst, their growths being *external* to the cyst.

There are some interesting and instructive features in this case, which, however, are so evident that they scarcely require notice. The origin of the disease, four years ago, from a violent blow;—this blow followed in six months by encysted dropsy of the ovary;—the temporary removal of this disease by iodine;—the apparent quiescence of the disease during the lapse of so long a period;—the suppression of the catamenia during this period;—its reappearance in October, and almost simultaneously with this reappearance, the lighting up afresh of this malignant disease, which ran its course so rapidly. All these are points interesting in a pathological point of view. Another point which is worthy of notice in the diagnosis of the case, was the apparently *unilocular* character of the cyst, whereas, in fact, the disease consisted of many hundreds of cysts. Both Mr. Babington and I were deceived upon this point, and the only way in which I can explain the illusion, is by supposing that, in consequence of the extreme thinness of the walls of the cysts, there was little or no resistance to the extension of the fluctuation from one cyst to the other.

Provincial Medical and Surgical Journal, July 11, 1849, *p.* 377.

141.—*Case of Ovarian Tumour containing Teeth and Hair.*—By W. Clapp, Esq., House-Surgeon to the Exeter Hospital.—[The patient was an unmarried woman, fifty-eight years of age, who had had a tumour in the abdomen, presumed to be ovarian, for twenty-eight years. The swelling having latterly increased very rapidly, so that respiration was impeded by the distension of the abdomen and pressure on the diaphragm, she was tapped, and about seven quarts of serum withdrawn. She died forty-eight hours afterwards, with

symptoms of exhaustion and peritonitis. Mr. Clapp gives the following account of the post-mortem examination:]

On opening the cavity of the abdomen a large quantity of effused fluid escaped: the peritoneum was found to be intensely inflamed and covered with recent lymph, and occupying the whole extent of the abdominal cavity was an immense tumour, which, arising from the right ovary by a narrow neck of about two-thirds the size of one's wrist, bulged out to double that size, and, again slightly contracting, immediately increased to the dimensions described, pushing upwards the stomach, liver, and diaphragm, and pressing the intestines backwards. It was firmly united by old adhesions on its upper and anterior surface to the omentum, and slightly so by recently effused lymph to the peritoneum lining the abdominal muscles. Its surface was of a dark purple colour, except in patches where it was thickened and white through former deposits of lymph: some of these were of considerable size, others smaller; and numbers of the size of millet-seeds existed on the intestines and other viscera of the abdomen. The tumour, on being removed, proved to be a cyst varying in thickness in parts, but generally about that of a shilling: its contents consisted of teeth, hair, bony deposit, some transparent masses of a cellular structure (as examined by the microscope), serum, sebaceous matter, and granular fat; which were contained in numerous smaller cysts. Teeth were found in all parts of the tumour, and were counted to the number of forty-three: some were contained in cysts; others imbedded in the semi-transparent masses, and two or three were growing from the walls of the parent cyst. In one part a few were imbedded in a mass of bone, bearing a strong resemblance to an upper jaw ununited in the mesial line.

The teeth consisted of molars, canine, and incisors, but in some parts two or three were united together, and one bore no resemblance to a human tooth. The bony deposit existed for the most part in small irregular portions and plates, without any definite form. The teeth and hair were examined by the microscope, and presented the normal appearance, except that the bulbs of the latter were imperfect. The sebaceous matter melted on the application of heat, and partially dissolved in ether. The uterus was of the size of a small pear, and, with the left ovarium and its appendages, of healthy appearance.

Medical Gazette, Aug. 17, 1849, *p* 282.

142.—*Account of a New Pessary.*—By Dr. C. Ritchie, Glasgow. —The uterine truss, when complete, consists of a frame, an abdominal belt, and certain narrow slips of India-rubber, attached to the belt and to the frame, by which the latter is kept in its proper situation in any position and state of the respiration. The frame consists of gutta percha, and of a slender wire of tempered steel, which runs enclosed within the

slightly curved vaginal stalk, from the elastic pessary at its uterine end, to the lower part of the upright hypogastric plate in front, and along the centre of each limb of each part of this for a couple of inches, when its ends are made to unite within a narrow cross band.

This frame may require slight modifications, in individual cases, as to the length of the vaginal stalk, the breadth of the pessary, and the breadth also of that part of the upright or hypogastric plate in front, which rests on the vestibule or nymphæ. It is possible, also, that the angle at which the pessary is placed with the vaginal stalk may sometimes require some variations; but I have found several modifications of the instrument suit equally well in the same case. This is to be ascribed to the flexibility of the instrument, by which it accommodates itself readily to existing circumstances.

In constructing this instrument, I limited myself to obtaining the needful support to the uterus consistently with what would not dilate the parts by its size, occasion pain by its hardness, or excite uneasiness by its weight, and I have thus necessarily got into the same general arrangement as to its shape with Dr. Simpson in his instrument. It will at once be obvious, however, that the principle on which the present truss supports the uterus is different from that of Dr. Simpson's. The pessary part of the frame is a very elastic oblong air-cushion, made of thin highly-polished gutta-percha, and may be made to rise at about an angle of 65 from the vaginal stalk, and, when the instrument is applied, to rest nearly parallel with the plane of the outlet of the pelvis, in apposition with the anterior aspect of the cervix and anterior labium of the uterus, thus pressing this organ, while it also supports it, in a direction backward and upward. The pessary can be attached to the vaginal stalk at a more acute angle, as in that represented in the wood-cut, in which it rises from the slightly curved vaginal stalk at an angle of about 45; and when in situ, and the woman in a standing position, its upper surface is nearly parallel with the plane of the brim, and the orifice of the uterus rests upon its centre.*

The shape of this pessary is otherwise very convenient, as one extremity of it, oiled, can readily, from its size and form, be passed through the narrowest sphincter, while the configuration of the vaginal stalk and hypogastric plate is such that when the former is fully introduced, even by the least intelligent patient the pessary must necessarily be in accurate apposition to the uterus.

The small diameter of the vaginal stalk, its elasticity and at the same time its strength, and the lightness and suppleness of the hypogastric plate, have commended themselves much to my patients.

I have said that the instrument is worn with an abdominal belt,

* The gutta-percha frames are made by Mr. Paterson of this place; and may be had of Mr. Hilliard here, or of Messrs. Hilliard & Son, Edinburgh. The price taken for them is four or five shillings. Mr. Paterson is well known as a scientific anatomical modeller.

to which it is attached by narrow tapes of India-rubber. The kind of abdominal belt may be allowed to vary, according to the wishes or wants of the patient. Some women, from the weight of the viscera of the abdomen on the uterus require a regular abdominal supporter, others may do with a band, three or four inches broad, of vulcanised caoutchouc. The smell of this latter is objected to by some, and its excess in elasticity by others, in which event cotton, linen, or flannel may be substituted.

The mode of attachment of the vaginal frame to this belt also differs in different cases. When a perfect abdominal supporter is worn, two slips of vulcanised India-rubber, each about a quarter inch in breadth, are sewed to the belt behind, corresponding in situation to each side of the coccyx. These are brought forward along the corresponding sides of the perineum, and made to cross beneath the transverse band on the front of the lower part of the hypogastric plate, which corresponds with the symphysis of the pubis, and their ends are then carried round the lower part of the trunk, and secured. Another pair of common or India-rubber tapes may next be attached to the upper corners of the hypogastric plate, and also carried round the body and tied. The lower edge of the abdominal belt covering the hypogastrium should then be brought down upon the front of the upright frame of the instrument in the vagina, and if necessary this edge of the bandage and the corresponding part behind may be kept secure by means of two additional India-rubber tapes passing along the perineum from one to the other.

When a simple band around the belly is all that is worn, it will be requisite, in addition to the tapes from behind, which are fixed, as in the former case, to the lower and upper angles of the upright hypogastric plate of the instrument, that the upper edge of this be enclosed either in a duplication of the lower edge of the abdominal band, if made of caoutchouc, or in that of a vertical two-inch broad piece of this material, attached by its upper edge to the band around the abdomen.

When either of these methods of affixing the uterine truss is employed, the vaginal portion of it is kept secure and comfortable in any position. The urine is evacuated without inconvenience, as are also the catamenia; and should either the edges of the abdominal supporter or of the perineal bands gall the skin, the evil is obviated at once by covering them with chamois leather.

The only other means of treatment I employ are—attention to prevent a loaded state of the bowels, and a regular use of the cold-water sitting-bath. This latter I employ at first at a temperature of 70 degrees, and for a few minutes only; afterwards, and in mild weather, I recommend the bath to be of a lower temperature, and to be used for half-an-hour.

Monthly Journal, July, 1849, *p.* 899

143.—ON THE USE OF CHLOROFORM IN MIDWIFERY.

By Dr. J. Denham, Ex-Assistant Physician to the Dublin Lying-in Hospital.

[The subjects to whom chloroform was administered by Dr. Denham, were generally healthy persons: some of the labours were natural, others tedious, or preternatural. Dr. D. says]

When a patient in labour is first brought under the influence of chloroform, it will generally be found that the pains are diminished in force, frequency, and duration. After some time they generally regain much of what they had lost, and though apparently weaker, they yet seem to tell with equal or even better effect upon the progress of the labour. The muscles of animal life are first affected, and if a complete state of anæsthesia be induced, the action of the uterus is also impaired. If the chloroform be suspended for a short time, both sets of muscles soon regain their usual power. The effect upon the sensorium is very variable; some pass rapidly into a state of insensibility, while others become boisterous and difficult to manage, but in all who can be induced to use it sufficiently long, it appears to mitigate the feeling of pain, to modify the impression, or remove the recollection of it. The pupils remain natural during the first or excitable stage, but when anæsthesia is finally induced they almost invariably become dilated. Of its effects upon the os uteri I cannot speak very confidently, as we seldom gave it in the first stage of labour; so far as I have seen, however, I believe it does not retard, but rather tends to promote dilatation of it. That the vagina and os externum become relaxed there cannot be a doubt. We did not remark the perinæum to yield more in proportion than the other soft parts. In no case did we witness injury to the child, even in the remotest degree.

I must say that I have never met with a single untoward circumstance affecting the health or life of either mother or child, that would in the slightest degree deter me from giving it where I thought it desirable or necessary. I have never witnessed any of the dreadful evils described by some writers as consequent upon its use; I have never seen the blood blackened, or the brain poisoned; nor has it ever, in my hands, induced convulsions, partial paralysis, or other still more formidable consequences; and until it does I will consider myself justified in still further investigating the subject, ever bearing in mind the maxim of Lord Bacon, that the labours of all who think, ought to be to multiply human enjoyment and to mitigate human suffering; and that, for this purpose, we must observe and reason only from what we see.

We have yet to give the result of our experience in preternatural, instrumental, and complex labours; and this part of the subject I approach with much more of pleasurable feeling, because that here I anticipate no difference of opinion, nor will we require either eloquence to adorn or argument to enforce the practical points to be elucidated; the mere recital of the cases will, I am sure, carry conviction to every candid mind.

Preternatural Labours.—Preternatural presentations of the upper extremities are at all times dangerous, as they call for a difficult and hazardous operation, involving, perhaps, the life of both mother and child; an operation which the most experienced practitioner undertakes with no small degree of anxiety, deeply impressed, as he must be, with the responsibility thrown upon him at such a time. It requires judgment and skill to know how to conduct such a case, when to interfere and when to remain passive; and he who undertakes it requires a clear head and a steady hand. How sad the fate of a patient in such circumstances who falls into the hands of a bold, untaught practitioner, who, ignorant of the dangers by which he is surrounded, and unmindful of the sacred charge committed to his trust, blindly rushes on to his own disgrace and the ruin of his hapless victim. How should we rejoice in the prospect of any remedy calculated to lighten the difficulties and lessen the dangers by which every such case is surrounded. Such an agent I believe chloroform to be; for if there is one operation more than another, in the whole range of midwifery, in which it is highly useful, it is that of turning; and if the discoverer of it accomplished nothing more, he has by this alone conferred a great benefit upon mankind, and largely earned the gratitude of his profession.

[After giving a series of cases of turning, in which chloroform was very successfully used, Dr. Denham says]

Such is our experience of chloroform in cases of turning; and so completely is the master of the hospital impressed with the value of it, that he administers it before turning as regularly as he should employ the perforator before extracting with the crotchet.

[On the subject of crotchet cases, Dr. Denham says]

On these cases in which delivery was effected by the crotchet, while the women were in a state of anæsthesia, we have scarcely a remark to make. The necessity for using it is not so great as in forceps cases, neither, perhaps, are the advantages so obvious, as the operation can generally be performed without much pain to the patient, and, in careful hands, with a very small amount of danger; at the same time, when we take into account the mental anxiety inseparable from such an occasion, and when we reflect how injurious any shock, mental or bodily, may then prove, I feel assured that we will seldom err in using chloroform, and that in the great majority of such cases it will be found a valuable adjunct in rendering the patient more manageable and the operation more easily performed, independent of any advantage it may possess with reference to ultimate recovery. In two of the crotchet cases, the patients were attacked with puerperal fever, one of whom died in hospital, and the other was forcibly removed by her friends.

In none of these cases had we hemorrhage to contend with, nor did any untoward symptom arise, either at the time of the operation or subsequently, that could be ascribed to the chloroform. There is one other point that I would here wish to direct attention

to, namely, the giving of ergot with chloroform. In some of the cases this practice was adopted, and in one or two with marked effect, as it produced a decided increase in the pains, though not sufficient to complete the labour. The same practice was adopted in other cases, not now under consideration, with the best results; and in no case did it seem to do harm. Combining ergot of rye with chloroform may, therefore, be considered both safe and judicious where circumstances call for it.*

[Dr. Denham next relates a series of forceps cases in which chloroform was given: he then remarks]

I have given a rapid outline of all the forceps cases in which chloroform was administered, and not only were the operations facilitated, but the sufferings of the patients greatly diminished, by its use; while the recoveries on the whole were above the average in such cases. In the two first-mentioned cases, either from the chloroform not being good (its purity being a most important point to attend to,) or from our want of experience in the mode of administering it at the time, the patients were not got into a state of perfect anæsthesia before the operation; we therefore experienced considerable difficulty from their restless opposition while in a state of semi-insensibility. In one of the twin cases there was an attack of hemorrhage after the expulsion of the placenta; but this I do not think could be ascribed to the chloroform. In the other twin case there was great difficulty in bringing down the head of the first child, a footling, and there was a considerable interval between the birth of that and the second. Was the chloroform the cause of this delay? Doubtless in some cases the pains are diminished in frequency and force, and may even be completely put a stop to during the state of anæsthesia, but they generally return with increased vigour as soon as the state of insensibility passes away.

[Lastly, Dr. Denham relates a series of cases of puerperal convulsions, in which chloroform was given in the intervals between the fits. He then proceeds to say]

These cases, I think, do not afford any evidence that chloroform is calculated to bring on convulsions, nor that it will increase the number or add to the severity of the fits when they have already set in. Our experience is as yet too limited to enable us to decide upon this point; but, with our present amount of knowledge, we would neither recommend it as a preventive or a cure for puerperal convulsions. If there was no organic disease present, nor any determination to the head in the intervals, we would not fear to administer it where circumstances seemed to indicate its use, or when called upon to perform the operation of turning, or delivery by instruments in a case complicated with convulsions.

From what I have seen of chloroform, I do not think that its

* Since this paper was read before the Obstetric Society, Professor Beatty confirmed the value of the practice by reading some cases of a similar nature to the Society.

use should be so restricted as Dr. Montgomery, in his valuable paper lately published in this Journal, would have us believe; nor have I witnessed the fearful results so graphically described by him as likely to follow from its use. Whilst I admit that some of the advocates for chloroform have been too lavish of their praise; on the other hand, I think that those opposed to it have been too unsparing in their censure. A middle course will be found the safest.

I consider it an agent powerful for good in many cases, yet in rash and inexperienced hands it may, and I fear often does, prove equally powerful for evil. That it may be and sometimes is given for the purpose of amusing patients, and making them believe that they are saved from a vast amount of pain, when in reality they have scarcely inhaled a single breath of it, I doubt not. If we give it fairly and honestly, we must be prepared to spend a much longer portion of time with our patients than we do at present, or intrust them, while in a state of anæsthesia, to the care of others during our absence. How any one in extensive practice can give up the time necessary for this plan of treatment I am at a loss to know, and yet some of our first and best accoucheurs boast that they use it in every case.

This report, I conceive, will bear us out in drawing the following conclusions:—

First. That chloroform is a most valuable agent in all turning, forceps, and crotchet cases.

Secondly. In some cases of natural or difficult labour it is useful in relieving pain, and in relaxing the soft parts, and thereby advancing the labour.

Thirdly. If given in large quantities, or if persevered in too long, it puts a stop to all muscular action. This is contrary to the opinion of Dubois, who states that it never destroys the uterine contractions, or those of the abdominal muscles.

Fourthly. In such cases the pains generally return with increased force when the chloroform is discontinued.

Fifthly. It does not relax the perineum more than the other soft parts. Dubois states that it does.

Sixthly. When organic disease does not contra-indicate its use, there is little danger to be apprehended from it in midwifery practice.

Seventhly. Chloroform may be administered to such a degree as not to take away reason and consciousness, and yet to secure the patient immunity from pain.

The mode of administering it in the hospital is by means of a hollow cone formed out of a piece of spongio-piline sufficiently large to cover the mouth and nostrils; to prevent it from collapsing, a few ribs of whale-bone are fastened on the outside, and over these there is a covering of fine leather. This method is both simple and efficient, as proved by our almost uniform success in producing the desired effect with a much smaller quantity of

chloroform than is generally used.* Professor Simpson says, that usually when the handkerchief is used, about an ounce per hour is necessary; some require less and some more. In one case lately, a first pregnancy, he states that he expended nearly six ounces in two hours. In the Dublin Lying-in-Hospital we rarely use more than one ounce in that length of time, and very generally less.

Dublin Quarterly Journal, Aug. 1849, *p.* 109.

144.—*On the Influence of Chloroform on Uterine Contraction.*—By Dr. Tyler Smith.—[Dr. Smith makes the following observations on the influence of chloroform upon the physiological contractions of the uterus:]

On rendering a guinea-pig insensible by the use of chloroform, the first things observed were, quickened respiratory movements, and convulsive actions of the limbs. The limbs were also forcibly retracted by reflex action, when pinched or irritated, notwithstanding the insensibility. Everything showed that the first effect of chloroform was to exalt the function of the spinal marrow. But, after a short time, the respiratory movements became languid, and the only parts which moved were the nostrils, larynx, and diaphragm, at considerable intervals. The sphincters of the rectum and bladder were relaxed. The animal was now perfectly quiet under external irritation, no reflex movements being excited. The abdomen was now laid open, the heart might be seen to beat; the peristaltic action of the intestines was excited by the stimulus of the air, or by the point of a needle; and the peristaltic contraction of the uterus was readily produced by irritation. These facts demonstrated that the organs, partly or wholly, under the influence of the ganglionic system, including the uterus, continued active after the reduction of the spinal function to a low point. Another experiment showed that chloroform had the power of destroying the functions of the spinal marrow even more rapidly than hydrocyanic acid. The animal being poisoned by chloroform, a stilet was passed into the vertebral canal, and the spinal marrow destroyed. At the destruction of the spinal marrow, no movements whatever of the limbs occurred. Under ordinary circumstances such a laceration of the spinal marrow would excite the most power-

*At the conclusion of Dr. Denham's report, Dr. Flemming exhibited and described an apparatus which he had constructed for the purpose of administering chloroform, and which he had used most extensively for some months past. This consists of a small capsule of glass, with an overlapping edge. The capsule is circular, and about two inches and a half in diameter. Within it is placed a small slice of sponge, which, when the apparatus is about to be made use of, is saturated with chloroform; and this he styles "the chloroform sponge." Round the neck of the glass a large cup-like sponge, hollowed out, and so shaped as to cover the nose and mouth, is attached. Through the pores of this sponge the atmospheric air is admitted. Before use, both sponges are damped with water, and then the proper measure of chloroform is poured upon the small flat sponge contained within the capsule.

ful contractions of the extremities. It was remarkable that, after this, a motor nerve being irritated, energetic muscular contraction was excited, so that the power in the nerves remained. Still, at the time when the functions of the spinal marrow, both reflex and direct, were thus destroyed, the uterus contracted under stimulus, after the occurrence of spinal death. These facts proved that the spinal marrow was affected by chloroform in the manner he had described. But, lest it should be supposed that the spinal functions ceased, not in consequence of chloroform, but from death itself, or the approach of death, he had repeated the experiment on the frog, in which it was well known that the reflex actions continued energetic for a considerable time after death. The frog being destroyed by chloroform, the reflex actions entirely and immediately ceased, the limbs remaining perfectly quiet under stimulation. The head of the animal being removed, a needle was passed down the spinal canal, and no movements occurred; but when the needle reached the lower part of the spine, the limbs contracted from irritation of the origin of the nerves. In a very short time, however, the power of the nerves ceased, even under the stimulus of galvanism, and the direct galvanic irritation of the muscles themselves caused very slight muscular contractions to take place. To apply these experimental facts to the condition of the parturient woman under chloroform: the first effect of chloroform upon the uterine action appeared to be stimulant; afterwards, the spinal functions diminished in activity, and the uterus was left in great measure to be emptied by the peristaltic action of the organ. Another point worthy of observation—namely, the occurrence of relaxation in the sphincteric orifices, the vagina, and the muscles of the floor of the pelvis, under chloroform. This relaxation was explained by the withdrawal of the influence of the spinal marrow. A power of contraction was taken away, but this was accompanied by dilatation of the resisting parts, so that, notwithstanding the deficiency of spinal contractile power, delivery was effected by the peristaltic action of the uterus. It was well known that delivery took place in the same way in certain cases of paraplegia, and even after death, but these facts by no means proved that the reflex function was not of the greatest importance in natural parturition. It appeared, then, that in deep anæsthesia, the reflex actions of labour were weakened in power, but that this was accompanied by dilatation or relaxation of the parturient canal, the peristaltic action of the uterus remaining little, if at all affected. These data were of importance towards arriving at a conclusion respecting the value of chloroform in natural labour, and those accidents, as hemorrhage or laceration, which depended upon increased or diminished motor power.

Lancet, Nov. 10, 1849, *p.* 508.

145.—*On Cauliflower Excrescence of the Os Uteri.*—By Dr. E. Watson, Glasgow.—[Dr. Watson thinks that the following considerations tend to prove that the cauliflower excrescence is not of a malignant nature:]

1. The progress of the growth is, generally speaking, slower than that of any known malignant disease with which we can compare it, such as encephaloid or colloid cancer, or of uterine hematoma. The early symptoms of the cauliflower excrescence are so very insidious, that it has generally existed for at least a year before medical advice is obtained, and even in cases where no treatment whatever has been adopted, the patient generally survives fully a year longer.

2. The usual age of the patient, in cases of cauliflower excrescence, is likewise different from that of the victims of malignant disease of the uterus. In nineteen cases of the former, the average age of the patient was thirty-seven, and no fewer than *eleven* were below it. Whereas, taking the nineteen first cases of cancer in Dr. Hughes Bennett's work on that disease, the average age is rather more than forty-three and a-half years. Boivin and Dugés, from very extended observations, found that the ordinary ages of patients suffering from malignant disease of the uterus, ranged from 35 to 55, and that those belonging to the latter decade were more numerous than those belonging to the former.

3. The cachexia attendant on malignant diseases, especially of the uterus, is generally absent from cases of cauliflower excrescence. In the last stages of this disease, the patient, doubtless, becomes pale and emaciated, owing to the constancy and profuseness of the discharge under which she is sinking, but it need hardly be remarked that such phenomena are very different from those of that premature decay which we denominate cachexia of malignant disease. It is an interesting corroboration of this remark, that, even in the last stages of cauliflower excrescence, the patient is capable of childbearing; and, in three of the cases formerly adduced, the patients not only gave birth to living children, but survived their labours for several months. I am not aware that the same could be said of cancer, or of any other truly malignant disease of the uterus.

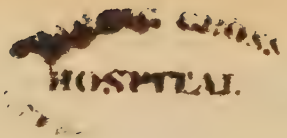
4. A not less remarkable distinction between the two classes of diseases which we are now considering, is that, during the whole course of the cauliflower excrescence, pain is almost never complained of by the patient. The discharge alone draws her attention to the disease; and when she is examined by the surgeon, it is found that the tumour itself is insensible to the touch. How different is such a state of matters from that which exists in cancer of the uterine neck. In the early stages of the latter affection, the cervix uteri is highly irritable and sensitive to the touch; and its first and last symptom is lancinating pain of almost intolerable severity.

5. It might be supposed that the microscope would lend important aid in the solution of this problem; but I fear it must be confessed that histological science has not sufficiently advanced to admit

of our relying with certainty on microscopic characters in the distinction of malignant and non-malignant tumours. The cancer-cell is the most trust-worthy sign of the former when it can be ascertained to exist in them, but this is by no means always an easy task; and even the most experienced microscopists have confessed their inability to distinguish it from certain other kinds of cells, such as from young plastic epithelial cells, or embryonal and cartilaginous cells. Little, then, can be argued from microscopical observations of the cauliflower excrescence in regard of its malignancy or non-malignancy, and even that little weighs in favour of the latter opinion. Only one out of four observers, viz., Dr. Anderson, saw "caudate corpuscles, "to use his own words," like those Müller saw in cancer and other bodies of various shapes." Mr. Lee says he saw "cells within cells or compound cells;" but these are by no means characteristic of cancerous growths, and, from his examination, he concludes that the cauliflower excrescence is "of simple structure, and not malignant." The whole tenor of Dr. Simpson's account of the excrescence examined by him indicates his opinion to be, that it is not a malignant growth, and he particularly remarks that he did not observe "any caudate or spindle-shaped bodies," such as are sometimes found in encephaloid structures. In none of the very numerous examinations made by me of all different parts of the cauliflower excrescence in my possession, was there detected any cancer-cell, or other body indicating malignancy of structure. I only found as formerly explained in detail, a considerable number of normal blood-corpuscles contained in a very delicate vascular membrane, and also a small quantity of fine cellular tissue in the centre of the growth. It will be perceived, then, that these microscopical investigations, so far from containing anything like a proof of the malignancy of this peculiar affection, rather tend, on the contrary, to establish the opposite opinion.

I have thus adduced cumulative evidence, warranting a conclusion, if not of absolute certainty, at least of the highest probability, that the cauliflower excrescence is not of a malignant nature; and that, therefore, we may advance to the removal of the growth with good hopes of a permanent cure of the disease. There can, however, be no doubt that such hopes depend for their fulfilment very much on the completeness with which the tumour is extirpated; and, with due deference, I beg to state it as my opinion, that the safest and most efficacious mode of accomplishing this end is by free excision of the affected lip, or, if necessary, of the entire neck of the uterus.

Monthly Journal, Nov. 1849, *p.* 1194.



ADDENDA.

146.—ON THE NATURE AND EFFECTS OF VARIOUS ANÆSTHETIC AGENTS.

By T. Nunneley, Esq., Surgeon to the Leeds Eye and Ear Infirmary, &c.

[A very elaborate paper by Mr. Nunneley has appeared in the Transactions of the Provincial Association, containing an account of a series of investigations undertaken by him for the purpose of ascertaining the following points:]

1. If the property of producing anæsthesia be confined to the few substances which have hitherto been used, or if there be not a large class of bodies which have in common the property of temporarily suspending consciousness or sensibility, or both, vitality remaining; and if this be so, whether these substances are characterized by any similar composition or chemical alliance, and have a common *modus operandi* upon animal bodies?

2. What their *modus operandi* is? or, in other words, upon what structures do they act? what changes do they produce? what are their physiological effects? is their primary effect local or general?

3. Is there any one of these substances which should be selected in preference to, or to the exclusion of, all the others? Is there any one, the temporary effects of which are more complete and certain, the permanent more harmless, and its administration more under our control, than those of any of the others, that we should uniformly retain the employment of it, and reject the use of them?

4. Is there any essential difference in the action of these substances according to the mode in which they are administered? Do they produce the same effects in whatever way they are carried into the body, or does the effect depend upon the nature of the tissue upon which they are applied? and does the same dose, when applied to the same tissue, and in the same manner, invariably produce the same uniform results?

5. Are there any symptoms by which we may accurately judge of their effects, and ascertain and control the point to which the system may with safety be brought under their influence?

6. In case of an overdose, are there any means which can be employed to counteract the effects? Do we possess any antidotes, or are there any remedies? If so, what are they, in what manner do they act, and how should they be employed?

[Mr. Nunneley observes:]

Nearly all of the substances which have hitherto been mentioned as anæsthetics, are compounds of the hypothetical radical ethyle—the assumed base of ether; substances which are formed of hydrogen and carbon united with oxygen, as an oxide of ethyle—sulphuric ether; or of this oxide, playing the part of a base, in combination with an acid, as nitric ether; or of hydrogen and carbon united with chlorine, instead of oxygen — chloroform, the most marked chemical character of these bodies being that they are all hydro-carbons. One other substance of somewhat analogous character has been mentioned, and shewn to possess, to some extent, similar properties, in which the hydrogen has been replaced by sulphur—the bisulphuret of carbon.

From reflecting upon this circumstance, I was, some time ago, led to the supposition that not improbably many other bodies might be found to possess similar powers, possibly even in a more advantageous form than some already known, since the use of these appears, so to speak, to have been rather accidental and fortuitous, than the consequence of any reasoning upon their nature and mode of operating. In fact, it appeared to me, that where hydrogen and carbon might be supposed to constitute an hypothetical radical or base, whether as ethyle, acetyle, methyle, or some other modification, they might be combined with another body; which third body might be replaced by another element, the properties of the compound remaining, to a great extent, the same. Thus, as ether is an anæsthetic, and composed of hydrogen and carbon, united with oxygen, which even may act as a base, and unite with an acid, one acid being capable of substitution in the place of another, the general character of an ether fluid remaining; and as chloroform is also an anæsthetic, in which the oxygen has disappeared and chlorine been substituted, so it appeared not only possible, but even highly probable, that this might be again replaced by iodine, nitrogen, sulphur, or some other element, the new compound in property being analogous, if not identical, with the old. That this is true, at least to some extent, is proved by the fact of hydrocyanic acid, a compound of nitrogen, carbon, and hydrogen, being a most powerful anæsthetic agent, perhaps the most powerful yet tried.

This, it will be at once perceived, is in some degree an extension of the beautiful theory of substitutions, now so generally admitted amongst chemists, applying the same doctrine to the therapeutic and physiological properties of isomorphous bodies, as has so long been known to prevail in their chemical qualities.

In pursuing the enquiry, I was led to endeavour to ascertain the truth of the converse proposition as to the different properties of bodies composed of the same elements in the same proportion, but possessing different chemical and physical qualities — isomeric bodies, of which the hydro-carbons under consideration form, perhaps, the most extensive class, many of them not differing materially in their ultimate analysis, while they are very diversified

in their physical qualities. However this may be, as we know too little of the manner in which many of these substances are actually united, notwithstanding the laborious investigations and ingenious hypotheses of chemists, it seemed only proper that other substances, composed mainly of hydrogen and carbon, in combination with another element, should be tried; accordingly the subjoined list of substances tried will be found to contain several of these; the number might be almost indefinitely extended. Some of the bodies with which I had a strong desire to have experimented, from their rarity and the difficulty of making them, I found it impossible to procure, many being described by chemists rather on hypothetical grounds, than from their actual existence. Others are so unstable in their composition, that it is impossible to preserve them; the affinity between their elements is so slight, that they are no sooner made than they are decomposed, hence they never could be practically valuable whatever their action; while others, from their irritating properties are useless. Some, as hydrocyanic acid and conia, are so destructive to life, as to be too dangerous to use; while, again, many others do not possess that property of volatility, or the solubility by which they can be made volatile, which is essential to their action; they are not isomorphous with the ether compounds.

Besides these triple compounds, I thought it not unadvisable to try the effects of some of the pure hydro-carbons, as naptha, turpentine, &c., and some of the bi-elementary gaseous bodies, as being most allied to the compounds which possess anæsthetic power, thinking this very power might possibly be dependent upon their containing these elements. One of these gases, if it did not actually suggest the idea to the introducers of these agents, certainly produces such effects, when inhaled by man, that, with our present knowledge, some persons have expressed wonder that it did not lead its adventurous and glorious discoverer, Davy, to the full development of the idea of which he undoubtedly had the conception. It affords another most striking illustration of the observation, that no great and wonderful truth all at once bursts upon the world, but that is always preceded by various and portentous foreshadowings, which, like the handwriting upon the wall, only require the skill of the prophetic seer, or the wand of genius, to develop to mankind.

Having ascertained that in order to possess anæsthetic powers, it was not necessary for a body to have a triple composition, but that some of the bi-elementary compounds also were endowed with it in a high degree, I was desirous of determining, if possible, if one element alone possessed the power, and if so, which of them; or if the combination of two or more be essential, of which in particular is the presence required. The only way to way to ascertain this was to exhibit, in succession, substances with one or more of the elements changed, it being impossible to present all of them in an uncompounded form, in that condition necessary for inhalation, or even for absorption; accordingly the list contains substances which some, at first sight, may be surprised at finding there. They were

examined, not only on their own account, but for the purpose of presenting bodies in which each of the elements entering into the constitution of a known anæsthetic was in succession removed, in order, by as exact an analysis as I could adopt, to examine each one element, and determine if it were essential or not to the possession of anæsthetic power; with what success we shall be better able to speak after going over the details. The difficulties in arriving at a positive conclusion are considerable; but I may mention the results prove that oxygen, chlorine, hydrogen, and nitrogen may be excluded, and yet we shall put an animal into a complete state of anæsthesia.

[After detailing at length, a large number of experiments made with various substances, Mr. Nunneley gives a summary of the action and properties of each. Some of the observations upon the more interesting anæsthetic substances, we quote; and first,]

Chloroform.—It is, of all the anæsthetics with which I am acquainted, (with one exception,) one of the most, if not the most, agreeable, and at the same time it is possessed of very considerable power. It is for the most part pretty constant in its action; but I am also bound to add, it is one of those which require more care in its administration than most of the others, and I would venture to say much more care than has always in practice been exhibited. It is true that many fatal cases in which it has been given are known, but, I am firmly convinced, there are many more fatal cases which have not been reported. Even in those cases which have come to light, all sorts of explanations, and attempts to explain away the true cause of death, have often been attempted, rather than admit the fact of the person having been killed by the chloroform. This is, perhaps, scarcely to be wondered at; the admission of the fact would be almost like acknowledging being accessory to the death of the patient, which most men naturally shrink from. A careful examination, however, of the preceding experiments cannot, I think, but convince any impartial person that, in reality, death in these cases was caused by the chloroform; either from some abnormal condition or peculiar idiosyncracy, by which the person was rendered unusually susceptible to its influence; or, as I believe, far more frequently from a want of that experience and care in its administration, which the exhibitor should have possessed and attended to. When I see statements by medical men that they have "boldly administered" chloroform in quantities of from six drachms to an ounce and a half at a time without any ill effects, I cannot but congratulate both the patient and the doctor that the rashness in employing such quantities has been equalled by the carelessness in administering it, so that by far the greater portion of the fluid has been wasted by escaping into the air of the room, rather than passed into the lungs of the patient, and thus the carelessness of the administrator has, contrary to what generally occurs, and which he would do well not to risk again, been the means of escaping from the effects of his rashness.

Two very important practical points, in reference to the administration of chloroform, appear to be proved by these experiments.

1st. That the effects produced upon similar animals by like quantities of chloroform, all circumstances, so far as can be ascertained, being precisely similar, are not identical, the effects being far more considerable in one than another: even upon the same animal the effects are far more decided at one time than they are at another.

2nd. That the intensity of effect appears much rather to depend upon the quantity of chloroform inhaled in a given time,—the concentration of the vapour, than upon the absolute quantity inspired. Thus a small quantity suddenly and quickly inhaled for a short space of time will produce, not only more rapid and decided effects, but also far more dangerous, than a far larger quantity will do, if taken in more slowly, and consequently in a less concentrated form. Many of the experiments show that when the creature inspired fully and deeply, and the temperature was high, so as to ensure a speedy conversion of the chloroform into vapour, that anæsthesia was not only quickly produced, but was also most decided; also that the effect was more prolonged if the animal recovered, and in many cases terminated in death.

There is one unpleasant effect which sometimes follows the administration of chloroform that I have not seen noticed, but of which I have found more than one person complain, namely, that for long (even months.) after its inhalation, they have occasionally experienced the same sensations as they did at the moment of breathing it. A highly respectable professional man (solicitor,) has told me that he regrets having inhaled chloroform, which he did many months ago, at Cheltenham, in the extraction of a tooth; since then he has on three or four occasions suddenly awoke in the night, with precisely the same feelings that he had just before becoming unconscious while inhaling it. He is conscious, but unable to speak or to stir, and feels as though about to die. After a time these sensations pass off, but leave him languid during the ensuing day. He never experienced anything of the kind before, and attributes them entirely to the chloroform. I have heard others say they regret having taken it, feeling nervous and more tremulous than before.

Sulphuric Ether.—If ether be not so powerful in its effects as chloroform, it is certainly not so dangerous in its application; a larger quantity is required to produce a like condition of insensibility, and more time is occupied; the system is not so speedily brought under its influence. In the commencement of the inhalation it is necessary to be very careful not to allow the air to contain too large a proportion of the vapour, otherwise so much irritation of the glottis and bronchial membrane is occasioned, and the respiratory muscles so violently excited to action, that the continuation of the inhalation must be, at least temporarily, suspended, sometimes even permanently. Another circumstance is, that before the animal is fully brought under its influence, there is some excitement, which, at times, is very considerable; so much so as to render a

patient wholly unmanageable, the only method of calming him being to place the system more fully, and as rapidly as may be, under the influence of the ether; when, however, this is accomplished, if not carried too far, so much depression is not produced as by chloroform, and I think the condition of insensibility may be longer continued without danger when ether is used than when chloroform is. The action of the heart is not rendered so excessively feeble and rapid, and disappears sooner than after chloroform.

To counterbalance this, feverishness, heat, and pain in the head, are more apt to follow. While the action of ether upon the system is, I believe, of essentially the same nature as chloroform, it, in the first instance, is certainly more of an excitant than chloroform, and, as stated, if not used in very large and unnecessary quantities, or very long continued, it does not occasion such depression of the system; hence, in cases where there is reason to suspect any peculiar susceptibility to the action of the depressing influence of anæsthetics,—such as disease of the heart, of an asthenic character, or considerable constitutional debility,—if it be determined to use one of these agents, ether should, I think, be selected in preference to chloroform. So also when the operation itself is likely to produce a considerable shock and depression of the system, or where it is likely to be very long and tedious, then it may fairly be well considered whether ether may not be the safer of the two agents. On the other hand, when the patient is plethoric, or very excitable, or in whom there is a tendency to fever,—where there is any affection of the head, whether of the brain or its membranes, or predisposition to congestion,—any sthenic disorder of the heart, or disease of any kind in the lungs; if it be, notwithstanding these contra-indications, from any other consideration determined to use any anæsthetic agent,—then ether is not the one to be selected, on account of its stimulating property.

Dutch Liquid—Chloride of Olefiant Gas.—This is not only an aromatic and very pleasant ethereal fluid, but a most valuable one; indeed, I believe one of the most valuable anæsthetics yet tried. In general properties it much resembles chloroform; but in composition it differs materially from it, in containing a much less proportion of chlorine and more hydrogen, as compared with carbon, in an atom; and its boiling point is very different, as a reference to the heads of the respective experiments will show.

A comparison of the experiments with chloroform and with the chloride of olefiant gas will show, that in no one respect is the former fluid superior to the latter. The animals were rendered perfectly anæsthetic in quite as short a time, showed no uneasiness while passing into this condition, remained perfectly still while in it, and on recovering were altogether free from any unpleasant symptoms. The condition of anæsthesia may be rendered as profound as possible, and quite as prolonged as can ever be requisite in any operation; and, so far as I can judge, there is not so much weakness and depression induced as by chloroform.

This fluid I have now administered so frequently in practice, that

I think I am justified in stating the inferences drawn from the experiments upon animals are correct. It appears to be unattended with the troublesome excitement produced by ether, on the one hand, and on the other with less of the tendency to collapse, which is so objectionable in chloroform; hence if that only true test, experience in general use, should confirm this opinion, it will form a not unimportant addition to the list of anæsthetic agents.

I would here state that it appears not improbable that a combination of the chloride of olefiant gas with spirit, not a mere admixture of the two fluids, but a combination during manufacture, may probably prove, not only a sufficiently cheap, but a valuable anæsthetic; for I have made several experiments upon dogs and cats with a mixture of pure Dutch liquid (made by Mr. Morson and Mr. Bullock,) and alcohol, and also of spirits of wine, in various proportions, the results of which I find to correspond with the effects stated to have been produced by a fluid which I now consider to be a combination of the two.

Common Coal Gas.—Coal gas is not only a powerful anæsthetic, but, so far as my experiments go, it is also a very safe and manageable one. The animal is quickly brought into a state of unconsciousness and insensibility, and that without the least appearance of suffering. It does not act as an irritant, it does not excite a cough, there is no repugnance to the inhalation of it, and the condition of anæsthesia may be safely maintained for a period sufficiently long to allow the performance of most operations.

It appears to deserve a full and fair trial, and if in practice it should be found, as I believe it will, to be successful, the cheapness of it would be a recommendation not unworthy of attention, should its gaseous form not prove too inconvenient. It affords another instance of a hydrocarbon producing an unnaturally bright condition of the blood.

Bromoform.—This fluid acts as an agreeable anæsthetic, without producing any irritation whatever, and might probably be successfully employed in practice, were it an article of easy production: but, inasmuch as it does not possess any striking superiority in any respect over other substances of a far cheaper character, its cost will be an effectual bar to its use.

[Besides giving these agents by inhalation, Mr. Nunneley has also applied them locally, and has administered them by the alimentary canal.]

Administration by the Alimentary Canal.—The experiments under this head, compared with others, are very interesting and important, as they prove most unequivocally that most, if not all, of these agents, when taken into the stomach or any portion of the alimentary canal, produce hardly any anæsthetic effect, acting at first as but little more than local irritants, unless the dose be very large; and even then, not producing so much effect as an equal quantity of spirit of wine. This is an effect which, *a priori*, hardly any one would have predicated, and which may assist in

throwing some light upon their action, and evidently depends upon some property in the substance itself, inasmuch as we find a considerable difference in this respect amongst them. Some act very violently and most speedily when applied upon the alimentary membrane, but with much less effect when applied by inhalation upon the respiratory membrane, as conia and spirit of wine. Hydrocyanic acid acts with great intensity, administered by either method, while the ethers, chloroform, Dutch oil, bisulphuret of carbon, (and especially sulphuretted hydrogen), produce, so to speak, hardly any effect when taken into the stomach, but a very decided effect when inhaled.

[Mr. Nunneley has ascertained that anæsthetic agents will produce local insensibility when *applied locally*, without the necessity of producing loss of consciousness. They cannot, however, be so employed in a large number of surgical cases, including the principal operations. Mr. N. observes]

In the smaller operations they certainly may be used so as to induce complete anæsthesia; and if there be any constitutional impediment contra-indicating the internal exhibition, as great susceptibility to their action, (as I have known in persons of a hysterical or cataleptic diathesis), there the local action should be preferred; but in other cases it is doubtful, if the longer time required for their application, and the smarting, which, if there be any abrasion of the skin, is great, (and if a wound of any extent, too great to inflict), which is occasioned, be worth undergoing for the sake of avoiding the inhalation; unless the part to be operated upon be small, I should say not.

There can, however, be no doubt that the local application of chloroform and other similar agents, in cases of severe pain, is often productive of the greatest comfort and relief. In painful and irritable tumours, the pain may often be alleviated. Tic douloureux I have seen disappear like magic, by rubbing the part with it, or saturating a piece of lint and covering it with oiled silk. Toothache may often be charmed away by a drop put within the decayed tooth. Lumbago and sciatica may be quieted by its use, and in many other cases the local action of chloroform has been very successful in relieving pain, as, indeed, great numbers of medical men, who have since the announcement of its local action tried it with manifest advantage, can attest.

[*Modus Operandi of Anæsthetics.*—Mr. Nunneley comes to the following conclusions upon this very difficult and intricate question:]

1. That the action of anæsthetic agents is immediately and primarily upon the nerves. That the heart, respiration, circulation, the blood, and the muscles are secondarily affected.

2. That in the first instance all these substances act as stimulants, this action being more or less prolonged, but to some extent perceptible in all. In the more powerful anæsthetics, as hydrocyanic acid, chloroform, and the chloride of olefiant gas, the stage of

stimulation is short and comparatively feeble, (unless with the two latter the dose be very small, when it is evident enough), the sedative effect being quickly superinduced. With most of the others it is more prolonged; with ether it is considerable; with aldehyde it is still more so; with the bisulphuret of carbon, turpentine, and naptha, even when inhaled, it is very great, and when applied locally, intense inflammation only is induced, anæsthesia scarcely being occasioned; while with alcohol, even when taken internally, unless the quantity be enormous, only the stimulant effect is produced at all.

3. That the action is at first local. When these agents in substance or in vapour are applied to a limited portion of the body only, that portion is rendered anæsthetic; but that if the application be more prolonged, or extended over a larger surface, then the effects become general. Thus we may paralyse a part, or the whole of a single leg in the frog and toad; by continuing the application, the corresponding limb becomes involved, and a still farther continuation will affect the entire body of the creature, consciousness, sensation, and motion being all lost. In a large insect, or a reptile, we may stupify the anterior or posterior half of the body with the attached limbs, the other part remaining unaffected. Even with the higher animals, the experiments demonstrate that, in the cat and rabbit, a part of a leg may be locally so affected as to be altogether insensible to the amputation of it, the general system not differing from its normal condition. In man, a small portion of the surface, or an entire finger, may be made insensible, the feeling of numbness extending along the nervous trunks far beyond the part to which the application is made. Hence we may fairly conclude that the law universally holds good.

4. That although the action of anæsthetic substances is upon the nerves, and principally upon those in connection with the cerebro-spinal axis, it is not primarily and directly upon the cerebral masses and sensorium, but upon the peripheral expansions of the nerves; that, *cæteris paribus*, the larger the number of these which can be directly and simultaneously acted upon, the greater will be the effect. Hence the *rationale* of the action of these substances when inhaled. When taken in substance into the stomach, they can only come in contact with a comparatively small number of terminal nervous papillæ, whereby is occasioned their stimulant, rather than their anæsthetic effect; but when inhaled, they come into contact with the whole expanded respiratory surface and the nerves there exposed. Of course the more concentrated the vapour, the more intense and sudden will be the anæsthetic effect.

[*Administration.*—Mr. Nunneley recommends us to use, for the exhibition of anæsthetics, "a large, moderately fine, linen handkerchief, folded first into quarter size, and then into a funnel shape." Mr. N. allows that the inhaler, as recommended by Dr. Snow, would be preferable, if, as that gentleman states, we could by this means accurately measure and regulate the quantity of vapour

administered. This, however, he thinks, would not be the case in practice. Mr. Nunneley thinks that it is a very common mistake to carry the anæsthesia too far: it is not necessary, he observes, to render the patient utterly *powerless*, in order that no sensation of pain should be perceived. With respect to the signs which indicate the degree of anæsthesia, Mr. N. observes:]

The first indication of an animal being pretty well under the influence of the agent is the deeper, fuller, and more prolonged respiration; he inhales as though it were pleasant to do so. This is not always observed, though it very frequently is. At first there is some little excitement, and a disposition to move; the pulse is rather fuller and quicker; the pupils somewhat contracted, and the eyes wandering; there is often incoherent talking; these symptoms speedily pass off, and loss of volition, and indisposition or inability to move, comes on; the eyes become upturned, the pupils dilated, the pulse smaller, the muscles flaccid, and unconsciousness and insensibility ensue. If the quantity of vapour be now lessened, the respiration becomes more free and natural: if not it will be laborious, and presently almost, or entirely cease. The lips become closed, and in expiration the cheeks may be distended by the air, which in escaping may produce a blowing noise. This I regard as a certain sign that the system is so fully under the influence of the anæsthetic, as to border upon danger. The distention and blowing noise arise, I think, from the total loss of motion and sensation in the lips and cheeks, which thus become mechanically distended by the force of the expired column of air. At this point, or before, (for the last symptom is by no means invariably present, the mouth being open instead of closed,) the pupils are widely dilated, the limbs become as flaccid as possible; there is no reflex action, the urine or fæces, or both, may involuntarily escape, the conjunctiva and lining membrane of the mouth, often, indeed, the whole countenance, become pale and bloodless; the heart may be perceived acting most rapidly, with a short, feeble throb, the pulsation in the arteries being scarcely, if at all, perceptible, and the respiration becomes either very quick and inefficient, or slow and gasping. If this entirely stops for any length of time, even though the heart may be felt still to move, recovery will be very doubtful. In operating, if the surgeon should find the flow of blood to cease, he ought *immediately* to be on his guard, as this may indicate such a condition of anæsthesia as to seriously compromise the heart's action; and if the inhalation be carried farther, might be fatal.

The indications, then, to be attended to in administering anæsthetics, especially the more potent, are,

Not to employ too much of the agent at one time.

To take care that the temperature be not too high.

To be sure that the air can easily and freely pass into and out of the lungs.

Not in the first instance to allow the vapour in too concentrated a condition to be inhaled, which is easily regulated by not putting the cloth too closely over the face in the first instance; or, if an

apparatus is used, to open the air valve. If there be no excitement, not to increase the strength of the vapour; but if there be excitement, then by applying the cloth more closely, or by shutting the valve, to terminate this by more fully and quickly bringing the system under the full influence of the agent, but taking care that so soon as there are indications of unconsciousness and insensibility being produced, in a great degree to withdraw the anæsthetic, and only to give such a diluted vapour as shall just suffice to keep up the condition of insensibility, which, when once produced, may be usually maintained by a very moderate quantity of vapour; for it must be borne in mind that the *full effect* is not always *instantly* produced as the vapour enters the lungs, nor do the effects always immediately pass off; indeed, the rapidity with which the effects disappear does not always correspond with their intensity. If these directions be attended to, rarely will death be occasioned by either ether, chloride of olefiant gas, or chloroform, although, as I have before stated, chloroform I consider to be the most dangerous of all: yet even of this, when due care is exercised, experience has shown the danger not to be very great.

[Lastly, Mr. Nunneley speaks of]

Remedies.—Various remedies have been suggested as possessing a controlling power over the effects of too great a dose of ether and chloroform; as yet their advocates can only advance assertion, or arguments based on theoretical grounds, in favour of their respective favourite means. From some of these means upon which most stress has been laid, if my ideas of the action of ether and other anæsthetic agents be correct, little good can be effected; but, nevertheless, it was only right to give them a fair trial. The remedies resorted to are ammonia, cold affusion, agitation, bleeding, galvanism, and oxygen; either alone, or in combination. In trying the effects of these, of course it was necessary to endeavour to bring the animal fully under the influence of the anæsthetic, otherwise by the mere cessation of the action of this, a natural resuscitation would ensue, and, possibly, value be attributed to the means tried to which it is not entitled.

[Mr. Nunneley regards *ammonia* as altogether valueless. He has the same opinion of *bleeding* in most cases. *Cold affusion* and *agitation*, he says, undoubtedly possess some power in hastening recovery, but not much; and the use of these measures is doubtful when the anæsthesia is profound, and consequently when the danger is the greatest. *Galvanism* is also useless. Respecting *oxygen*, which has been so much extolled by some, Mr. Nunneley remarks:]

In recommending several of the so called remedies and antidotes, one most important circumstance appears to have been altogether overlooked, namely, that in the cases where their employment is advised, there is no means of administering them. Deglutition is destroyed, consequently fluids cannot easily or safely reach the stomach, and respiration has ceased, or nearly so, hence gaseous

matters cannot be inhaled, even were they at hand; for the administering such gases by artificial respiration would be very difficult and imperfect. This is supposing it were certain that they are as valuable as has been supposed, which I believe not to be the case. If oxygen, or any other gas is to act as an antidote, it must be by decomposing the chloroform or other agent in the lungs; but as oxygen does not decompose these fluids out of the body, it is not likely to do so within, even did these fluids remain any time in the lungs, which they do not. To act as a remedy, it must be either by directly destroying the action which the poison has occasioned, or by setting up an opposite action which shall counteract it. Now as we have no evidence that the action of chloroform and allied anæsthetics in any way depends upon the abstraction of oxygen from the blood or tissues, or prevents the exhalation of any noxious matter which the presence of oxygen favours, there is no reason to suppose the presence of this body will counteract or destroy the influence of them.

The opinion which appears fairly deducible from reasoning alone, is fully borne out by facts. So far as the experiments with oxygen go, they show it to be altogether useless; indeed, if any conclusion is to be drawn from them, it would rather be that oxygen, instead of being beneficial, is injurious.

[Mr. Nunneley, in conclusion, makes the following practical observations upon the means of counteracting the effects of an overdose. He says:]

In the great majority of cases, so long as the heart acts and respiration at all goes on, the mere withdrawal of the anæsthetic is followed by the restoration of the functions, and therefore the question forces itself upon us as to the positive value or not of any of these means. If the heart has ceased to act no blood will flow on a vein being opened. If respiration has ceased, and the heart does not move, galvanism, though it may make the voluntary muscles violently contract, only does this by the sooner exhausting their their power to do so, without in any way restoring the vital functions. Oxygen, of course, if there be no respiration, cannot be taken into the lungs, and the performance of artificial respiration with it would be both difficult and incomplete, even were it at hand, which it would not in the great majority of cases be, while, so far as experiment and chemical action goes, it does not appear that if it were ever so fully inhaled, even in combination with the anæsthetic substance, it would in any manner prevent the development of full anæsthesia, and therefore it cannot be for a moment imagined it would in any way counteract the effects when they are developed. Ammonia we have seen to be useless and inert, whether given mixed with the substance or after its effects were produced. The coldness and depression which in so marked a degree are present do not encourage us in the supposition that either cold water or cold air, to a very great extent, would be useful—a conclusion which the experience of their effects confirms. We are, therefore, reduced to seek just that assistance from their use as shall suffice

to produce such a moderate shock, if the nervous system be capable of receiving any, as may cause a reaction and rousing of its powers, by which the heart and respiration may again be set in motion, or rendered more vigorous. This, as I have before said, appears far more likely to be accomplished by the sudden dashing of a small quantity of cold water upon the face and chest, possibly alternating with heat to the latter, or such a moderate stream of air as may be produced by a fan, and a not too violent agitation of the body than by excessive violence, or by entire and long-continued immersion. A moderate interrupted compression of the chest, and pressure upwards of the abdominal viscera, should not be neglected, as tending to change the air in the lungs, and thus to get rid of the vapour there remaining; or, with the same intention, a stream of air might be, not too forcibly, blown through a small tube into the larynx for a minute or two.

These are remedial means which some will, I doubt not, be disposed to exclaim are very paltry and meagre, and which, it must be allowed, are not of a very energetic or hopeful character: but it is far better to employ such as may do a little good than to have recourse to those which not only may, but probably will, do great harm.

Provincial Medical and Surgical Transactions, Vol. XVI., 1849, *p.* 367.

147.—*On the relative Potency of Chloroform and Ether, and on some dangerous Effects of Chloroform.*—By Dr. John Snow.—Chloroform is much more powerful than ether, and this is one reason why it is in some respects more convenient. Its greater potency depends on its being more sparingly soluble in the blood than ether. The quantity of chloroform required to induce insensibility is less than one tenth as much by measure, as in the case of ether. Viewed in this manner it is more than ten times as strong; but to ascertain their comparative physiological power, when inhaled in a similar manner, their volatility requires to be taken into account. In order to perceive the relative strength of these two medicines, we may suppose that the air which a patient breathes is saturated at 60°,—the ordinary temperature of a dwelling room,—with one or other of the vapours, and see how much air he would have to breathe in either case, in order to be narcotized to the third degree,—the extent of insensibility usually required in a surgical operation. Thirty-six minims is about the average quantity of chloroform required to produce this degree of narcotism in the adult, and this would saturate 257 cubic inches of air at 60°, making it expand to nearly 300 cubic inches, which would be breathed in 12 ordinary respirations of 25 cubic inches each. The quantity of ether usually required to produce the same amount of insensibility in the adult, is about $7\frac{1}{2}$ fluid drachms; this would saturate 440 cubic inches of air at 60°, and increase its volume to rather more than 800 cubic inches, which would require 32 ordinary respirations to breathe it. We see, therefore, that 12 inspirations of air charged with vapour of chloroform

are equal to 32 similar inspirations of air charged with vapour of ether, at the same temperature; and that, consequently, chloroform is nearly three times as strong as ether. In actual practice, the difference in strength is generally greater than this, for ether abstracts much more caloric than chloroform during its evaporation, thereby reducing the temperature of the air passing over it, and the sponge or whatever contains it, and limiting its own evaporation in a greater degree. It follows, therefore, that the fact of accidents not occurring under the use of ether, could be no guarantee that they would not happen during the employment of chloroform.

When an animal after it has become completely insensible, is allowed to continue breathing air charged with the vapour, the respiration shortly ceases; but if the air do not contain more than about 5 per cent of the vapour, the heart continues to pulsate for some time after the breathing has ceased, and the circulation is finally arrested for want of the respiration, as in all other cases when death takes place by apnœa. I have heard the pulsations of the heart, by means of the stethoscope, several times, for one or two minutes after the breathing has ceased, in cats and rabbits under the influence of chloroform. During this interval, life is easily recalled by means of artificial respiration; indeed, more than once, moving the animal, or pressing on its chest, whilst using the stethoscope, has apparently been the means of resuscitating it. This persistence of the heart's action, as I have elsewhere shown, does not arise from any incapacity of chloroform to paralyze it, but from the circumstance that the sensibility of that part of the nervous system on which the motions of respiration depend, is abolished by a somewhat smaller quantity of the narcotic than is requisite to suspend the action of the heart. When, however, an animal is made to breathe air containing a greater quantity of the vapour, ten per cent. or upwards, death takes place rapidly in from half a minute to about two minutes, and the respiration and circulation cease about the same time. The reason of this is, that there is sufficient vapour in the lungs, at the moment when the breathing stops, to paralyze the action of the heart as soon as it is absorbed, and added to that already contained in the blood. Under these circumstances, it is evident that artificial respiration can be of no avail; and this is the manner in which, there is every reason to believe, the greater number of the fatal cases of inhalation of chloroform have occurred.

Sometimes a patient begins all at once to breathe deeply during the inhalation; and, under these circumstances, if the vapour be not largely diluted, it will be inspired with dangerous rapidity. The first incision by the surgeon's knife, when the patient is unconscious, but not totally insensible, sometimes has the effect of causing him to draw a deep inspiration, and to hold his breath at the end of it, retaining the air in his lungs; now an inspiration of this kind, might, on a moderate computation, introduce 100 cubic inches of air; and if this were charged with vapour of chloroform, by passing over a handkerchief or sponge, it might contain

ten or twelve minims; if the air or the handkerchief were warm, it might, indeed, contain much more; but this quantity added to that already in the circulation, might cause a fatal accident.

It must be sufficiently evident, from these considerations, that, unless some means were used for regulating the strength of the vapour, fatal accidents would be liable to occur from the employment of chloroform. Unfortuately, Dr. Simpson, to whom we are indebted for its introduction, recommended it to be used on a handkerchief, and even held it out as one of the advantages of the new anæsthetic, that it did not require any apparatus. This advice, coming from so high a quarter, could not fail to meet with numerous followers; and to this circumstance, many of the accidents that have occurred must, in my opinion, be partly attributed.

Edinburgh Medical and Surgical Journal, July 1849, *p.* 75.

148.—*Account of a Case of Death from Chloroform.*—By Samuel Solly, Esq., Surgeon to St. Thomas's Hospital.—John Shorter, aged 48, a porter, known to Mr. Solly for some time as a very active messenger, habits intemperate, but apparently in perfect health, was admitted into George's ward, under Mr. Solly, on the 9th October, 1849, suffering from onychia of the left great toe, which had existed some time. It was determined to remove the nail, the man having decided before entering the hospital on taking chloroform.

On Wednesday, Oct. 10, at a quarter to 2 p.m., he began to inhale the chloroform with one drachm in the inhaler. It had no visible effect for about two minutes: it then excited him, and the instrument was removed from his mouth, and about ten drops more were added; he then almost immediately became insensible: the chloroform was taken away, and the nail removed. He continued insensible; and, his face becoming dark, the pulse small, quick, but regular, respiration laborious, his neckerchief was removed, and the chest exposed to fresh air from a window close to the bed; cold water was dashed in his face, the chest rubbed, and ammonia applied to the nose. After struggling for about a minute, he became still, the skin cold, pulse scarcely perceptible, and soon ceased to be felt at the wrist; respiration became slow and at intervals, but continued a few seconds after the cessation of the pulse. Immediately on the appearance of these symptoms, artificial respiration was commenced by depressing the ribs with the hands and then allowing them to rise again until the proper apparatus was brought, when respiration was kept by means of the trachea-tube and bellows, and oxygen gas introduced into the lungs by the same means. Galvanism was also applied through the heart and diaphragm, but all signs of life ceased about six or seven minutes after the commencement of inhalation. These means were persisted in until a quarter past 3, but to no purpose. On removing the inhaler, the sponge, which only contains one drachm, fell upon the floor, and the chloroform splashed about,—thus showing that a consider-

able part of the chloroform remained unused; so that the patient could not have inhaled more than a drachm. Every endeavour was made to procure a post-mortem examination, but in vain.

Medical Gazette, Nov. 2, 1849, *p.* 757.

149.—*Case in which a very small dose of Chloroform was efficient.*—By Dr. T. P. Heslop, Resident Medical Officer to the Birmingham General Hospital.—[In this case the chloroform was administered to relieve the pain produced by the passage of a gall stone. Dr. Heslop tells us that]

Thirty minims were accurately measured, and placed upon the sponge of a simple inhaler. The inhaler was scarcely adjusted when she was sound asleep; certainly no more than one second elapsed before complete sleep was induced, altogether without prior excitement and muscular spasms. There was no very obvious alteration of the circulation. The inhaler was instantly withdrawn, and in about a minute she awoke. She began to utter cries of pain again as before the chloroform was inhaled. The inhaler was again adjusted, without any more chloroform being placed upon the sponge, and she was again, in about two or three seconds, put into a profound sleep, without the slightest previous excitement. On this occasion the inhaler was kept on the mouth a longer time, and therefore she remained asleep several minutes the pain being completely suspended. On re-awaking, and again complaining of pain, I put ten minims upon the sponge, which had been previously moved to and fro in a current of air. This appeared to produce only uneasy sensations; fifteen minims were added, when she began to exhibit the muscular spasms and excitement ordinarily attendant upon the exhibition of chloroform in the first stages of its action. This woman was in the recumbent posture during the whole time As far as we know, the cerebral, respiratory, and circulatory systems are healthy. Surely this is powerful proof of the importance of attending upon idiosyncrasy in the administration of this remarkable agent; and as we have no means of discovering this in a particular patient before the chloroform is given, it must be concluded that it behoves us to commence, as a rule, with the minimum dose.

Medical Gazette, June 15, 1849, *p.* 1035.

150.—ON THE PROCESSES OF REPAIR AND REPRODUCTION

By James Paget, Esq., Professor of Anatomy and Surgery to the Royal College of Surgeons.

While studying the means by which living bodies, having suffered injury, regain their perfection, we may find continual manifestations of the same mysterious properties of the germ by which, through development, they first attained it. And in these manifestations we may discern the best evidence that, in developing the

body to its perfection, the power of the germ is not lost, or quite exhausted, but rather is diffused through all the parts of the completed being: and that, so diffused, it works in them through all their life, determining, as with continuous design, every natural formative process, in accordance with the specific character of the individual. For I believe that we cannot form a just conception of the scope and nature of even the least of the processes of repair and reproduction after injury, or of the maintenance of the body in ordinary nutrition, or of its natural changes in the course of age, unless we admit that each organism, in its perfect state, retains, diffused through all its parts, some of the specific properties from which the power issued that actuated the impregnated germ in its development.

The characteristic property of an impregnated germ is, that when placed in favourable circumstances, all the materials of which it first consists, and all that it appropriates, are developed according to the same method as was observed in the development of its progenitors,—in other words, in conformity with what we may regard as a law of specific character. In all the wonders of development that my colleague, Mr. Owen, has detailed, none, I think, appeared more marvellous than the constancy, the seeming tenacity of purpose, with which the germ is thus developed to the likeness of its parents. However vast its power of multiplication and increase,—however various its metamorphoses,—however far in some of these changes it may deviate from the form in which its parents generated it,—however near in some it may approach the perfect characters of another species,—or, which is stranger still, however much alike all germs may be in their primal structure and earliest developments,—yet, through all these things, each germ moves, with unswerving progress, guided by the same Power as created its first parents, to the formation of a being in which the parental form and properties are reproduced.

Now, the constancy of this result, and its little dependence on external circumstances, justify the expression that every impregnated germ has, in itself, and in the properties with which its Maker has endowed it, the power to develope itself into the perfection of an appropriate specific form. However mysterious the nature of such properties, we cannot deny their existence, or refuse to recognise a law (in the sense in which we generally use that term,) in the regularity with which the power acts that issues from them, when the germ is placed in favourable conditions. And, therefore, if it appear that this power is peculiar in its modes of operation, we may specialize it, whenever acting, as the "power of the germ," or "germ-power," in consideration of its having its apparent origin and intensest action in the germ.

The accepted doctrine in physiology seems to be, that each structure in the body has the power of taking from the blood, by a kind of elective affinity, certain appropriate materials, and of so influencing them that they assimilate themselves to it; i.e. they adopt or receive its form and properties, and incorporate themselves with it; or else it is held that each cell or structural element of a part,

while developing itself into some higher form, leaves behind or produces germs, cytoblasts, or off-shoots, which shall pass through the same development as itself, and in due time succeed to its place and office.

But since we see that the continual mutation of particles for the growth and maintenance of the living body by nutrition manifests in all essential things, the peculiar features that characterized the first formation of the same body from the germ, we seem justified in holding that it is one and the same power which, being maintained continuously from the germ to the latest period of normal life, determines all organic formation. Whatever be the properties of the germ from which this formative power emanates, they must, in due measure, be communicated to all the materials that the germ appropriates, and, successively, to all that enter into the construction of the developing body; so that in all its living parts there is a measure of the same power as was most vividly concentrated in the germ. Under this power, the organic assimilative force, in some instances, and the production of successive tissue-germs in others, appear to work as subordinate agents; but in many instances, as in the formation of blood, the power seems to act more directly upon amorphous organic matter, which in the perfect body, as in the germ, it informs according to the law of specific character.

If what has been said appear sufficient to prove the operation of the germ-power, in the ordinary maintenance of the body, i.e. for the reproduction of particles which are absorbed or cast off at the term of their natural life, then more than sufficient evidence for the same opinion will appear in the phenomena of repair and reproduction after injuries. These, occupying, as it were, a middle place between the phenomena of embryonic development and those of ordinary nutrition, have of late been alternately, or in different instances, referred to one or the other of the different powers by which those phenomena are supposed to be governed. But, probably, the likeness of the phenomena of reproduction to those of both nutrition and embryonic development should have suggested, as they did to Blumenbach and Treviranus, that all the three are manifestations of one and the same power. That they are so will be evident, I think, in what I shall have to describe in the following lectures. For, 1st. it will be clear that repair and reproduction cannot be in any case explained as the result of assimilation or by the succession of tissue-germs. In grave injuries and diseases, the parts that might serve as models for the new materials to be assimilated to, or as tissue-germs to develope new structures, are lost or spoiled; and yet the effects of injury and disease are recovered from, and the right specific form and composition are regained. And, secondly, if we watch the manner in which the recovery is achieved, we shall discern in it all those characters which mark the operations of the power of the germ. We shall see that the reproduced parts are formed, not according to any present model, but according to the appropriate specific form, and often with a more strikingly evident design towards that form, as

an end or purpose, than we can discern in the natural construction of the body. We shall see, too, that in this re formation there is an independent, though concurrent, development of may parts; and that the whole process is limited by the same restrictions of time, and space, and exact method, as those observed in the original development of the parts. This, also, may be added to the arguments already adduced to prove the continuous action of the germ-power through life, and its slight variation therein; that when in an adult animal, a part is reproduced after injury or removal, it is made conformable, not only to that condition which was proper to it when it was first formed, or in its infantile life, but to that which is proper according to the time of life in which it is reproduced—proper, because like that which the same part had, at the same time of life, in members of former generations. In the reproduction of the foot or the tail of the lizard, they grow, as it were, at once into the full dimensions proper to the part, according to the age of the individual. Spallanzani expressly mentions this:— that when a leg is cut off from a full grown salamander, the new leg and foot are developed, as far as form and structure are concerned, just as those of the larva were; but as to the size, they from the beginning grow and are developed to the proper dimensions of the adult. The power, therefore, by which this reproduction is accomplished, would seem to be, not the mere revival of one which, after perfecting the body, had lapsed into a dormant state, but the self-same power which, before the removal of the limb, was occupied in its maintenance by the continual mutation of its particles, and now engages itself, with more energy, in the reconstruction of the whole.

Our ideas of the consumption of power in the organization of matter, are, perhaps, unavoidably, very vague; yet there are facts enough to prove that the power which can issue from the mysterious properties of a germ is limited; that it is not ever communicated to an indefinite quantity of matter; and there are enough to justify the hypothesis, that the germ power, thus limited is in some measure consumed in the development of every new structure, and, in a less measure, in the growth and maintenance of those already formed. The instances to which I shall refer are explicable on this hypothesis, and are, therefore, evidences of its probability.

First, then, it appears constantly true, that the reparative power is greater in all parts of the young than of the older individuals of all species. Even when we compare individuals that have all attained their highest development and growth, this rule seems to be true. We know it from general observations of the results of similar injuries and diseases in persons of different ages; numerous as the exceptions may be, the general rule seems true. And it is yet more evidently proved in the case of some lower animals. Spallanzani mentions it expressly in regard to the reproduction of the tail of the tadpole. The quickness with which the work of reproduction is both begun and perfected, was always, in his experiments, in an inverse ratio to the age of the tadpole. He says the same for the

reproduction of the legs of salamanders, and it is only in young frogs and toads that any reproduction of the limbs will take place. So, too, in the experiments on the repair of fractures, the union of tendons and the like, in the mammalia; one may see abundant evidence that the vigour and celerity of the process are in an inverse proportion to the animal's age. There is, indeed, some reason to believe, that in the very early period of embryonic life, a true reproduction of parts of limbs may take place, even in the human species; (for not to speak of the possibility that supernumerary members may sometimes have been formed in consequence of accidental fission of the budding limbs of the embryo), there are some cases in which fingers are found on the stumps of arms in such circumstances as justify the belief that after a limb had been accidentally amputated in the uterus, these had been produced on its remaining portion.

All these facts agree well with the belief that the germ-power is gradually diminished in the acts of organizing matter for the maintainence of the body; and the difference between the reparative power in children and that in adults appears so much greater than the difference in adults of different ages, that it is probable the germ power is more diminished by growth than by mere maintenance.

However this may be, it seems certain that the capacity for the repair or reproduction of injured parts is much more diminished by development than by growth or maintenance of the body; i. e., much more by those transformations of parts by which they become fitted for higher offices, than by the multiplication or maintenance of those that are already perfect in their kind and function. In other words, to improve a part requires much more, and more perfect, formative power, than to increase it does.

This, as a general principle, is exemplified in many instances. In the greater part of congenital malformations we find arrest of development, but no hindrance of growth; as a heart, in which a septum fails to be developed, yet grows to its full bulk. So, if tadpoles be excluded from due light and heat, their development will be much retarded, but their growth will be less checked. So, too, in the miscalled cultivation and improvement of flowers, growth is increased, but development is hindered; and an excess of coloured leaves is formed, instead of the due number of male and female organs. Or again, in an old ulcer or a sinus, cells may be continually reproduced, maintaining or even increasing the granulations, yet they will not develop themselves into cellular tissue and cuticle for the healing of the part; and so, even when repair and reproduction have gone far towards their ultimate achievement, that which takes a longer time, and oftener fails, is the improvement, the perfecting the new material, by its final development. This is observed in all cases of reproduced limbs, and even in ordinary scars.

These facts (and there are many others like them) seem to justify the expression that, not only more favourable conditions, but

also a larger amount of organizing force, is expended in development than in growth. If we may thus interpret the facts, they will be collateral evidence for the belief that, in different species of animals, the reparative power will bear an inverse ratio to the amount of development already passed through; so that, for each species, in its perfect state, the reparative power might be measured by the degree of likeness between the embryonic and the perfect form, structure, and composition. The greater the sum of dissimilarities in all these respects between the embryo and the perfect animal, the less seems to be the reparative power in the latter.

It is consistent with this that the highest amount of reparative power exists in those lowest polypes, in which, as my colleague has so ably argued, the materials of the germ-mass yet remain, not transformed, but multiplied, and, as it were, grouped into the shape of their bodies. In the fresh water Hydra, the power of the germ appears communicated without loss to all the cells that descend by multiplication from the original germ-cells; provided only the cells are not developed into higher forms or into subserviency to special functions, In the hydra viridis, and hydra fusca, it seems literally true that any minute cluster of cells, derived by mere multiplication from those of the germ-mass, may, after having been separated from the perfect body, reproduce the perfect form. This is the general truth of the numerous experiments performed on hydra by Trembley, Roesel, and others. They have been so often quoted, that I need not do more than mention the greatest instances of reproductive power that they showed.

Trembley cut an hydra into four pieces: each became a perfect hydra; and while they were growing, he cut each of these four into two or three. These fractions of the quarters being on their way to become perfect, he again divided these, and thus he went on, till from the one hydra he obtained fifty. All these became perfect; he kept many of them for more than two years, and they multiplied by their natural gemmation just as much as others that had never been divided. So that a division by fifty did not perceptibly diminish the power of the germ present in the polype that was thus subdivided. Again, he cut similar polypes longitudinally, and in an hour or less each half had rolled itself, and seamed up its cut edges, so as to form a perfect hydra. He split them into four—he quartered them,—he cut them into as many pieces as he could, and nearly every piece became a perfect hydra. He slit one into seven pieces, leaving them all connected by the tail, and the hydra became seven-headed, and he saw all the heads eating at the same time. He cut off the seven heads, and, hydra-like, they sprang forth again. And even the fabulist dared not invent such a progidy as the naturalist now saw. The heads of the Lernæan hydra perished after excision: the heads of this hydra grew for themselves bodies, and multiplied with as much vigour as their parent trunk.

Now these instances may suffice to show, that in the lowest polypes the process of reproduction after injury confounds itself with that of their natural generation, by gemmation, or, as it probably

more rarely happens, by spontaneous fission. We can not discern a distinction between them; and there are some things which seem to prove the identity of the power which operates in both. Thus, in both alike, the formative power is limited according to the specific characters of the hydra; immense as the power of increase is, which may be brought into action by the mutilations of the hydra, yet that power cannot be made to produce an hydra of much more than ordinary size, or to raise one above its ordinary specific characters. And again, the identity of the power is shown in this,—that the natural act of gemmation retards that of reproduction after injury. Trembley particularly observes, that when an hydra, from which the head and tentacula had been cut off, gemmated, the reproduction of the tentacula was retarded soon after the gemmule appeared.

The instance of the hydra is only one of many proving it to be a general law, that a large power of repair and reproduction after injury exists in the species in which propagation by spontaneous fission, or by gemmation, is possible. I need not say how consistent this is with Professor Owen's doctrine—that Partheno-genesis, or the capacity of generating without previous impregnation of the parent, is dependent on a retention of a part of the original germ-cells, or of their direct and untransformed descendants, within the body of the parent. Wherever such cells are, there is the power, without the need of impregnation, of producing a new individual; and in all the instances in which such cells form part of the general substance or parenchyma of the body, an injury may be said to act only as a stimulus, exciting the premature exercise of a ready power, which, in the natural case, appears as the generative, but, in the accidental case, appears as the reparative power.

I need not enumerate the species which manifest this coincidence of the power of propagating by gemmation, or fission, and of reproducing large portions of the body, and even of reconstructing, from fragments, the whole body. They include, as chief examples, the Actiniæ, which after bisection form two perfect individuals,—and the Holothuriæ, which, as Sir J. G. Dalyell has observed, when hurt or handled, will eject all their viscera, leaving their body a mere empty sac, and yet in three or four months will have all their viscera regenerated. They include, also, from among the Anellata, the young Nereids, and those species of Nais, on which Bonnet, Spallanzani, and others, made their marvellous experiments,—experiments of which the climax seemed to be achieved when a Nais was cut by M. Lyonnet into thirty or forty separate pieces, and there were produced from those fragments as many perfect individuals.

Instead of dwelling on these instances, let me mention some observations, made by Sir J. G. Dalyell, upon other polypes, which seem calculated to illustrate in a remarkable manner the general laws of the reparative processes in even the higher animals.

In the Hydra Tuba,—the species of which he has traced that

marvellous development into Medusæ,—he has found that when cut in halves, each half may regain the perfect form; but this perfect form is regained only very slowly, and, as it were, by a gradual improvement of parts that are, at first, ill-formed. The fact may possibly be explained (as he suggests) by the mutilation having disturbed the progress of the Hydra in its development of young Medusæ; for the experiment was made in March, nearly at the time when the series of changes should have commenced. But, if I may venture not to accept the suggestion of so admirable an observer, I should suspect rather that this is an instance of gradual recovery of perfection, such as we see more generally in the repair of injuries and diseases in the higher animals.

He has noticed something of the same kind, and more definite, in the Tubularia indivisa. A fine specimen was cut near its root, and after the natural fall of its head, the summit of its stem was cloven. An imperfect head was first produced, at right angles to the stem, from one portion of the cleft; after its fall, another and more nearly perfect one was regenerated, and, as it grew, improved yet more. A third appeared, and then a fourth, which was yet more nearly perfect, though the stem was thick, and the tentacula imperfect. The cleft was almost healed; and now a fifth head was formed quite perfect; and after it, as perfectly, a sixth and seventh head. All these were produced in fifteen months.

The lower half of this specimen had been cut off four months after the separation of the stem. Its upper end bore—first, an abortive head; then secondly, one which advanced further in development; a third, much better; and then, in succession, other four, which were all well formed.

The upper portion of this lower half of the stem now shewing signs of decay, a portion was cut from its lowest part, and further manifested the reproductive power of the stem; for three heads were produced from the upper end of the piece cut off, and four from the lower end of the upper piece which had seemed to be decaying. In 550 days this specimen had grown 22 heads.

Now, I cannot but think that we have, in these instances of gradual recovery from the effects of injury, a type of that gradual return to the perfect form and composition, which are noticed in the higher animals. Our theory of the process of nutrition leads us to believe that in the constant mutation of particles in nutrition, those elements of the blood, or of any structure that have been altered by disease, in due time degenerate or die, and are cast off or absorbed, and that those which next succeed to them partake, through the assimilative force, of the same morbid character; but that, every time of renewal, the new particles, under the influence of the germ-power, approach a step nearer to the perfect state. Thus, as it were, each generation of new particles is more nearly perfect, till all the effects of the injury or the disease are quite obliterated. Surely, in the gradual recovery of perfection by these

polypes, we have an apt illustration of the theory,—one which almost proves its justice.

The power of reconstructing a whole and perfect body, by the development of a fragment, is probably limited to the species that can propagate by spontaneous fission or gemmation, or that increase their size, as some of the Anellata do, by the successive addition of rings that are developed after the manner of gemmules from those that precede them. Where this power is not possessed, there, whatever be the position of the species in the animal scale, the reparative power appears to be limited to the reproduction of lost members,—as legs, claws, a part of the body, the head, an eye, the tail, and the like: yet, within this limit, the rule seems again to hold good,—that the amount of reparative power is in an inverse ratio to that of the development, or change of structure and mode of life, through which the animal has passed in its attainment of perfection, or on its way thitherward.

Here, however, even more than in the former cases, we need, not perhaps more experiments, but experiments on a larger number of species. It appears generally true, that the species whose development to the perfect state is comparatively simple and direct, have great reparative powers; while many, at least, of those in which the development is with such great changes of form, structure, and mode of life, as may be called metamorphosis, retain in their perfect state scarce any power for the repair of losses. Yet we want more instances of this; and especially, it were to be wished that we had the results of experiments upon the lowest animals that pass through such metamorphoses, e. g. on the Hydra tuba, not only in its hydra state, but in all the changes that succeed, till it attains its complete Medusa form.

In the absence of such evidence as experiments of this kind might furnish, the best examples of this rule are furnished by the experiments of Mr. Newport. They show that among the insects, the reparative power, in the complete state, is limited to the orders in which that state is attained by a comparatively simple and direct course of development—as the Myriapoda and Phasmidæ, and some of the Orthoptera. These can reproduce their antennæ, and their legs, after removal or mutilation; but their power of reproduction diminishes as their development increases,—and, even in the Myriapoda, whose highest development scarcely carries their external form beyond that of the larvæ of the more perfect insects,—even in these such reparative power apparently ceases, when, after the last casting of their integuments, their development is completed.

In the higher hexapod insects, such reproduction has been seen in only the larval state,—none of them, in its perfect state, can reproduce an antenna, or any other member. The Myriapoda, then, are, in their reparative power, equal to the larvæ of the higher insects, and nearly all the germ-power which these manifest, appears to be exhausted in the two later metamorphoses.

The case is the stronger, as illustrating the expenditure of germ-power in metamorphoses, when the higher insects are compared

with the arachnida; for in these, which attain their perfect state through more direct development, the reparative power remains equal to the reproduction of limbs and antennæ. A yet stronger contrast is presented between the higher insects and the several species of salamander in which so profuse a reproduction of the limbs has been observed; for though they be so much higher in the scale of animal life, yet the amount of change in external form and habits of life through which they pass in their development from the embryo to the perfect state, appears less than that accomplished in the metamorphoses of insects.

Many instances, besides those which I have cited, appear to support this rule, that the reparative power in each perfect species, whether it be higher or lower in the scale, is in an inverse proportion to the amount of change through which it has passed in its development from the embryonic to the perfect state. And the deduction we may make from them is, that the power for development from the embryo is identical with that exercised for the restoration from injuries: in other words, that the power is the same by which perfection is first achieved, and by which, when lost, it is recovered.

This is, again, generally confirmed in the instances of the vertebrata; but of the repair in these, or at least in the highest of them, I shall have to speak so exclusively in the future lectures, that I will now only say that, in the highest vertebrata, and in man, a true reproduction after loss or injury seems limited to three classes of parts:—

1. To those which are formed entirely by nutritive repetition,—such as the blood and the epithelia.

2. To those which are of lowest organization, and, (which seems of more importance) of lowest chemical character,—as the gelatinous tissues, the cellular and tendinous, and the bones.

3. To those which are inserted in other tissues, not as essential to their structure, but as accessories, as connecting or incorporating them with the other structures of vegetative or animal life,—such as nerve-fibres and blood-vessels.

With these exceptions, injuries or losses in the human body are capable of no more than repair, in its more limited sense; i. e., in the place of what is lost, some lowly organized tissue is formed, which fills up the breach, and suffices for the maintenance of a less perfect life.

Let me express my belief that, if ever we are to escape from the obscurities and uncertainties of our art, it must be through the study of those highest laws of our science, which are expressed in the simplest terms in the lives of the lowest orders of creation. It was in the search after the mysteries—that is, after the unknown highest laws —of generation, that the first glance was gained of the largest truth in physiology—the truth of the development of ova through partition and multiplication of the embryo-cells. So may the study of the repair of injuries sustained by the lowest polypes lead us to the clearer knowledge of that law, in reliance upon which

alone we dare to practise our profession,—the law that lost perfection may be recovered by the operation of the power by which it was once achieved. Already, in the facts that I have quoted from Sir Graham Dalyell, we seem to have the foreshadowing of the facts through which the discovery may be made.

And let us not overlook those admirable provisions which we may find in the lives of all that breathe, against injuries that, but for these provisions, would too often bring them to their end before their appointed time, or leave them mutilated to complete a painful and imperfect life. We are not likely to undervalue, or lose sight of, the design of all such provisions for our own welfare. But we may better appreciate these if we regard them as only of the same kind as those more abundantly supplied to creatures whom we are apt to think insignificant: indeed, so abundantly that, as if with a consciousness of the facility of repair, self-mutilation is commonly resorted to for the preservation of life. When the Ophiura, or any of the brittle star-fishes, break themselves to fragments, and disappoint the grasp of the anxious naturalist, they probably only repeat what they are instinctively taught to do, that they may elude the jaws of their more ravenous enemies. But death would be better than such mutilation, if their rays could not be reproduced almost as easily as they can be rejected. The experimentalist, too, who cuts off one or the other end of any of the Annellata, perhaps only puts them to a necessity to which they are liable from the attacks of their carnivorous neighbours. Almost defenceless, and so easily mutilated, their condition, were it not for their faculty of reproduction, might be more deplorable than that of any other creature; and even their existence as species might have been endangered long ago. It would almost seem as if the species that have least means of escape or defence from mutilation were those on which the most ample power of repair has been bestowed;—an admirable instance, if it be only generally true, of the beneficence that has provided for the welfare of even the least (as we call them) of the living world, with as much care as if they were the sole objects of the Divine regard.

[In proceeding to speak of the healing of divided soft parts, Mr. Paget observes:]

Recent improvements in surgery have shown how right Mr. Hunter was, when, in the very beginning of his discussion concerning the healing of injuries, he points out, as a fundamental principle, the difference between those forms of injuries of which one is subcutaneous, the other open to the air. He says, "The injuries done to sound parts, I shall divide into two sorts, according to the effects of the accident. The *first* kind consists of those in which the injured parts do not communicate externally, as concussions of the whole body, or of particular parts, strains, bruises, and simple fractures, which form a large division. The *second* consists of those which have an external communication, comprehending wounds of

all kinds, and compound fractures." And then he says "The injuries of the first division, in which the parts do not communicate externally, seldom inflame; while those of the second, commonly both inflame and suppurate."

In these sentences Mr. Hunter, has embodied the principle on which is founded the whole practice of subcutaneous surgery; a principle, of which, indeed, it seems hardly possible to exaggerate the importance. For of the two injuries inflicted in a wound, the mechanical disturbance of the parts, and the exposure to the air of those that were covered, the exposure, if continued, is the worse. Both are apt to excite inflammation: but the exposure excites it most certainly, and in the worse form; i.e., in the form which most delays the process of repair, and which is most apt to endanger life. Abundant instances of this are shown in the difference between a simple and a compound fracture, though the former may have been produced by the greater violence; or, between a simple fracture, even with much violence, extending into a joint, and an open wound, never so gently made into one. Or, for parallel instances, one may cite the rarity of suppurations after even extensive ecchymoses, and their general occurrence when wounds are left open.

[Mr. Paget states, as the probable ground of this difference, that the materials produced for the repair of open wounds are either different, or do not develop themselves in the same manner, as those for the repair of subcutaneous ones. Before speaking, however, of the materials of repair, he states that the healing of open wounds may be accomplished in five different modes, namely:]

1. By immediate union; 2. By primary adhesion; 3. By granulations; 4. By secondary adhesions, or the union of granulations; 5. By healing under a scab. The repair of subcutaneous wounds may be effected by immediate union, but is generally accomplished by connection, or the formation of bonds of union between the divided and retracted parts. Very rarely it is effected by means of granulations without suppuration.

[Mr. Paget thinks that Mr. Hunter was in error in supposing that union by the first intention is effected by the organization of the fibrine of the blood effused between the surfaces of the injured part. He gives the following account of the way in which blood thus effused may be disposed of:]

In my last course of lectures, I stated the evidence for believing that masses of effused, or stagnant and coagulated blood may be organized; i.e., may assume the characters of a tissue, and may coalesce with the adjacent parts and become vascular. Among these evidences, several cases were mentioned in which such organization of the blood had been distinctly traced; they included cases of blood effused in serous sacs, especially in the arachnoid, clots in veins organizing into fibrous cords, or, after less organization, degenerating into phlebolites, clots organizing into tumours in the heart and arteries, and the clots so organized above ligatures on

arteries as to form part of the fibrous cord, by which the obliterated artery is replaced. These last cases afforded the most conclusive evidence, because they had been very carefully investigated in a series of experiments and microscopic observations, by Dr. Zwicky. In the course of last year I had the opportunity of examining a specimen which, more fully than any other I had seen, confirmed his account of the mode in which blood-clots become organized. It supplied, too, some facts which appear to me important to the present subject.

The specimen was obtained from an insane person, by my friend Mr. Holmes Coote. A thin layer of pale blood-coloured and ruddy membrane lined the whole internal surface of the cerebral dura mater, and adhered close to it. Its colour, the existence of patches of blood-clot imbedded in it, and all its other characters, satisfactorily proved that it had been a thin clot of blood,—an example of such as are effused in apoplexy of the cerebral membranes, of such as Mr. Prescott Hewett has so fully described in his paper in the Medico-Chirurgical Transactions. Numerous small vessels could be seen passing from the dura mater into this clot-membrane. The arrangement of blood-vessels bears a close resemblance, but, perhaps, more in its irregularity than in any positive characters or plan, to that which exists in false membrane formed of organized lymph; but the vessels were, I think, generally larger.

Such were the vessels of this organized clot. Its minute characters which are of peculiar interest because of their resemblance to those observed in the material that is commonly formed in the repair of subcutaneous injuries. In the substance of what else appeared like a filamentous clot of fibrine, sprinkled over with minute molecules, the addition of acetic acid brought into view corpuscles like nuclei, or cytoblasts, very elongated, attenuated, and, in some instances, like short strips of flat fibre. Of course, such corpuscles are not to be found in any ordinary clot of fibrine; neither are they seen in any stage of the organization of such lymph as is commonly effused in inflammation; but they exactly resemble such as may be found in certain examples of rudimental cellular tissue, and (among these) in the material for the repair of subcutaneous injuries. In short, the minute structure of this clot now organized was an example of what I shall have often to refer to under the name of "nucleated blastema."

With such evidence as this of the organization of a thin layer of blood-clot, and of the development of its fibrine being apparently identical with that of the material commonly formed for the repair of subcutaneous injuries, it was with much surprise that I found that extravasated blood can, commonly have no share at all in the repair of injuries.

We have sufficient proof that extravasated blood is not necessary for union by the first intention, or for any other mode of repair, in the simple fact that where the repair is best, and the

material for it most ample, no blood is so extravasated as to form a clot that could be organized.

But, though this may be the usual case, the question still remains—When blood is effused between wounded surfaces, and clots there, how are the clots disposed of? For often, though not generally, such clots are found in wounds, or between the ends of a broken bone, or a divided tendon when an artery by its side is cut; and in most operation-wounds, one sees blood left on them, or flowing on their surfaces, after they are done up. How, then, is this blood disposed of?

Ejection and absorption are, doubtless, the usual means by which blood in injuries is disposed of; yet I feel nearly sure it may in some instances become organized, and form part of the reparative material.

On the whole, I believe we may thus conclude concerning the part that blood, when it is extravasated, takes in the repair of injuries:—

1. It is neither necessary nor advantageous to any mode of healing.

2. A large clot, at all exposed to the air, irritates and is ejected.

3. In more favourable conditions the effused blood becomes enclosed in the accumulating reparative material; and while this is organizing, the blood is absorbed; and—

Lastly, it is probable that the blood may be organised and form part of the reparative material; but even in this case it probably retards the healing of the injury.

[Mr. Paget next proceeds to speak of the characters of coagulable lymph. He says]

Regarding its vital properties, the essential character of the coagulable lymph is its tendency to develope itself,—a tendency which it has of its own properties. It thus displays itself as a plasma or blastema—a fluid to be classed with those others that manifest the capacity to assume organic structure,—such as the lymph and chyle that develope themselves to blood; the semen, which, at first fluid, gradually developes itself into more and more complex structures.

The natural tendency of coagulable lymph is to develope itself into the fibrous, or the common fibro-cellular or connective, tissue, —the lowest form of vascular tissue, and the structure which, in nearly all cases in man, constitutes the bond by which disunited parts are again joined. This is commonly formed, whatever be the tissue upon which the lymph is placed, whether containing cellular tissue in its natural structure or not. This, therefore, we may regard as the common or general tendency of lymph; but in certain cases the development of lymph passes beyond this form, or deviates from it into another direction, in adaptation to the special necessity of the part to be repaired. Thus, for the repair of bone, the lymph may proceed a certain distance towards the development of fibrous tissue, as if for a common healing; but this fibrous tissue may next ossify; or not forming fibrous tissue at all, the

lymph may proceed at once to the formation of a nearly perfect cartilage, and this may ossify. In general, moreover, the character of the connective tissue that is formed in repair is adapted to that of the parts that it unites. The bond for the union of a tendon is much tougher than that for a common scar in the skin; that in skin is tougher and less pliant than that in mucous membrane, and so on.

In its first production, the reparative material is like the lymph of inflamed serous membranes,—at least, no characteristic difference between them is yet known; neither are there yet any observations to show a difference in the characters of the materials effused for the repair of injuries of different parts, or in different circumstances; and yet such a difference, in even the original characters of the lymph, is indicated by the fact that in different circumstances it may proceed to the same end—the formation of fibrous tissue —by two different ways of development. The lymph, or new material, which is produced for the repair of open wounds, generally developes itself into fibro-cellular tissue through nucleated cells; that formed for the healing of subcutaneous wounds as generally developes itself into the same tissue through the medium of nucleated blastema.

Now both these are repetitions of natural modes of development of the same forms of tissue. In the embryo and fœtus you may trace very well the development of subcutaneous cellular tissue through nucleated cells, and that of tendons and other formed fibro-cellular or fibrous tissues through nucleated blastema.

It must not appear an objection that there should be two modes of development to the same perfect tissue; for this is usual, and has been observed in nearly all the tissues. I spoke of it last year in relation to the development of the blood-corpuscles, of which a first set are formed from part of the embryo-cells that form the germinal area, or the whole body of the embryo, and the second set are formed exclusively from the corpuscles of lymph and chyle; and so it is with the cartilage, the muscular, and other tissues that are formed in the earliest periods of embryo-life. At first they are developed from some of the embryo-cells; yet in later life, no such cells are seen among them, but others appropriate to them, and of widely different form. So also in the bones, which at first are developed through cartilage, but in their subsequent growth are increased by ossification of fibrous tissue; and in the repair of which we shall find even more numerous modifications of these different developments.

The general truth appears to be (as already stated) that the material of repair for subcutaneous wounds is developed into cellular tissue through the formation of nucleated blastema; while that for repair by primary adhesions, and by granulation, is developed into the same through nucleated cells. Now, since both these modes of development are, as I have already said, imitations of natural methods, we might suppose that they are therefore both alike natural or healthy processes,—both alike independent of

inflammation,—alike sure to pass to their proposed end, safe from disease or degeneration.

But, if we consider also the morbid conditions in which these two methods of development of cellular or fibrous tissue occur, then we can see that the development through cells is characteristic of inflammatory process—at least, as far as the origin of the cells is concerned. For, of such cells, in various stages of development, are formed not only long-suppurating granulations, but also the walls of abcesses, inflammatory infiltrations, producing succulence, indurations, and thickenings of soft parts; the lymph produced in inflammation of serous membranes, which organizes itself into false membranes.

On the other hand, the development of cellular tissue through nucleated blastema, is, in morbid processes, observed in instances in which there are commonly no signs of inflammation,—as e.g, in warts and condylomata, in the simple fibro-cellular tumours of the subcutaneous tissue, in nasal polypi, in organizing clots of blood.

These instances, then, may enable us to conclude respecting the existence or non-existence of inflammation in the healing of injuries. The material of repair in union by primary adhesion and granulations is the same, in appearance and mode of development, as we find produced in other conditions which all are agreed to call inflammatory. But, in the healing of subcutaneous wounds, we find a different material of repair, which resembles, in both appearance and mode of development, that which we find forming growths whose production is not attended with any signs of inflammation. On these grounds, therefore,—and they are corroborated by the appearances of the injured parts during the process of healing,—we are justified in holding, generally, that inflammation ensues in the healing by adhesion and by granulations, but does not exist in the healing of subcutaneous wounds.

Although inflammation may be deemed necessary for the production of the lymph that may form adhesions or granulations, yet we have no evidence that the continuance of inflammation is necessary for the organization of the lymph. Rather, the persistence of inflammation seems to hinder the organization of the lymph, to keep it in a low state of development, or to favour its degeneration. Thus, to name but one example, granulations over an irritating body suppurate abundantly, and remain for years unhealed;—that is, they are imperfectly organized; they stop short of their development into the cellular tissue and cuticle proper to cicatrices, or degenerate into pus.

I may also add, that, although I have described the two modes of development of fibro-cellular or fibrous tissue for the healing of wounds as if they were always as separate as they are distinct, yet they may co-exist, and probably often do so. In the repair of many wounds, the two materials—namely, that which is to be developed through nucleated cells, and that whose progress is to be through nucleated blastema—may be mixed. Thus, in subcutaneous wounds and injuries—as, for example, in the case of divided tendons,—the first

consequence of the mechanical violence is the effusion of a common inflammatory product, which makes the cellular tissue œdematous, and organizes itself into nucleated cells. Thus you find the space between the retracted parts for about two days. But, then, the more proper and purer material of repair is produced; and this increasing in an inverse proportion to the degree of inflammation, soon overwhelms the former product of inflammation, and is developed into the nucleated blastema. Still, for many days, traces of the inflammatory product may be discerned mingled with the blastema, confusing its appearance, but, I believe, finally organizing with it into the bond of union. So, in divided muscles, and in simple fractures, the inflammatory exudation, produced in consequence of the first violence, appears to mingle and develope itself with the more proper material of repair; but they bear an inverse proportion to one another, and the more manifest the signs of inflammation, the less is the quantity of the proper reparative material, and the slower, in the end, the process of repair.

On the other hand, I think that in the ordinary healing of open wounds, which are soon brought together by sutures, or other appropriate means, there may be less than the commonly observed formation of nucleated cells, and some of the reparative material may be developed through the nucleated blastema. Or, when the materials are not mingled at the same spot, yet, in one wound, different parts may be healed by the organization of one or other material, according to the degree of inflammation that is in each part present during the production of the lymph. But all these things need further inquiry.

Medical Gazette, June 22, 1849, *p* 1064.

[We regret that our limits will not permit us to quote Mr. Paget's most interesting description of the five modes (before enumerated) in which open wounds may heal. We proceed to the subject of the *union of fractures.* The descriptions usually given in surgical works, of the mode in which fractures are united, are derived mainly from experiments on animals; but, Mr. Paget states, the mode of union in man differs considerably from that in animals. He says:]

It is probable, that the difference between the modes in which fractures are commonly united in man and other animals repectively, depends in part on the movement to which the fragments are subjected in the latter, — but probably, in part also on the greater readiness with which, under all circumstances, bone is formed in the animals lower than man.

Since the above was written, I have examined many more specimens, and find the same rule true—namely, that in the ordinary repairs of simple fractures in the human subject, the reparative material, or callus, is merely inlaid between the several fragments; it fills up the interspaces between them and the angles at which one fragment overhangs another; but it does not encircle and ensheath them, in the manner implied in the description of provisional callus;

nor is it in any considerable quantity, if at all, deposited either beneath the periosteum or within the medullary tube. In birds, dogs, and other ordinary subjects of experiments, the formation of a provisional, or, as it may, perhaps, be better called an *ensheathing* callus, is usual.

For fractures in the human subject, the evidence that union is accomplished by the reparative material being placed between, not within and around, the fragments—i.e., as an intermediate, not an ensheathing callus—this evidence may be obtained by the examination of such fractures even long after they are completely healed. In as many as you like to examine you will find the new bone formed exclusively between the fragments. Whether they were in apposition, or nearly so, or wide apart, still there is no appearance of new bone being formed on the outer side of any fragment,—I mean on that side which is turned away from the other fragments. And this is the case even in those instances in which there is so much displacement of the fragments, and so much distortion, that we can hardly suppose the repair to have proceeded very quietly. Neither in any of these do you find new bone within the medullary tube. It may be objected by some to these specimens, that the fragments were once ensheathed and blocked up with callus, and that it has been since absorbed. But this is not probable, seeing that in many cases there remain, on the outer surfaces of the fragments, certain marks of their original form and slight irregularities. In one of the specimens, we have traces of the healing of a long fissure, which appears now as a sunken groove, making it nearly certain that no new bone was formed over it. In another, is a detached piece of the wall of a femur turned quite round, so that its periosteal surface lies on the periosteal surface of the principal fragment: yet on the outer surface of this piece (which was the inner surface of its wall) the thin plates forming the boundary of the medullary tube are still unchanged.

Such appears to be the natural mode in which the reparative material is deposited for the union of fractures of human bones. And, regarding the particular position which it may in each case occupy, I do not know that it can be more exactly described, than by saying, that it is deposited where it is most wanted for the strengthening of the bone,—so that, whatever would be the weak part of the bone, if unhealed, there is the new material placed, in quantity as well as in position just adapted to the exigencies of the case, and restoring, as much as may be, the original condition and capacities of the bone.

The exceptions to the rule of difference in the repair of human bones and those of animals, confirm it as thus explained; for the only bones in which, in the human subject, a provisional callus is generally or naturally formed, for the repair of fractures, are the ribs. In cases of fractured ribs one may see, indeed, a very close imitation of that which is described, from experiments on animals, as the ordinary mode of union. The provisional callus is well formed under the periosteum, and encircles, like a broad ring or

ferrule, both the fragments, and may almost completely ossify before their union is accomplished, or even apparently begun.

Another bone for the repair of which, but more rarely, callus is formed around the fragments, is the clavicle; and the best specimen in which I have seen it is one in which the fracture was not detected, and the fragments were allowed to move on one another, till the patient died twelve weeks after the injury.

Except in such cases as these of fractures not kept at rest I doubt whether a natural formation of callus beneath the periosteum, or within the medullary tube of a human bone, would ever occur. In disease, the occurrence is not so rare; for, when the natural process of union fails altogether, the loose ends of the bones may be inclosed within a case formed wholly or in part of bone; or an imitation of callus may be made by a gradual morbid accumulation of bone around a fracture, even after its natural union.

But I think the comparative restlessness of animals is not alone sufficient to account for all the difference in the processes. The remainder may be ascribed to their greater tendency, in all circumstances, to the formation of new bone. Not in fractures alone, but in necrosis this is shown. It is very rarely that such quantities of new bone are formed in even children, as are commonly produced after necrosis of the shafts of bones in dogs or other animals; nor is there in the human subject any such filling up of the cavities from which superficial sequestra have been separated, as the experiments of Mr. Hunter showed, after such exfoliations from the metatarsal bones of asses.

Other examples might be quoted, but these may suffice to show that, after injuries, new bone is formed more abundantly in animals than in man. And I hope enough has been said to prove that the generally received account of provisional callus, and other parts of the healing of fractures, is an exaggeration of what occurs in man. If it be asked what it is that is felt like a callus after fractures, I would say that, in such cases as I could examine after death, I have usually found that the overlapping ends of the bone, being both at once grasped, had been taken for the enlargement of callus. Sometimes, also, the thickening and induration of the parts around the fracture infiltrated with serous and bloody fluid, or with lymph, have been mistaken for it.

The next point on which I proposed to speak, concerned the structure of the reparative material thus deposited within the fragments.

It will appear that the peripheral growth and ordinary maintenance of all bones, after the ossification of their primordial, and for a time growing, cartilage is completed, is effected by the ossification of membranous or fibrous tissue. I would call it fibrous rather than membranous, because in some instances, as the patella and other sesamoid bones, and in the union of fractures, and the growth of some tumours, a mass of tissue, rather than a membrane, ossifies.

The changes that ensue directly after a simple fracture are in

accordance with what I have already described in relation to the general repair of injuries. A period of rest follows the injury; and for a week or ten days no change may be observed, at least in fractures in the adult long bones: and here, at once, is a point of contrast with the consequences of fracture in the lower animals; for specimens, showing the consequences of fracture of the tibia in rabbits, six, ten, and fourteen days after the injury, prove that a perfect cartilaginous callus is in them formed in a shorter time than would elapse before the commencement of any distinct reparative process in man.

Especially, for contrast, one may notice the unchanged state of the periosteum round the broken human bones: for this, in ordinary cases, is neither raised by deposit beneath it, nor in any way altered—except, it may be, by slight thickening—during the whole healing of the fracture.

The first new material produced after simple fracture consists of the lymph and serum, which are effused in consequence of the inflammation that the violence of the injury excites. But these, as in the repair of other subcutaneous injuries, are presently overwhelmed and enclosed (as the effused blood also is) by the more proper reparative material. In a plan, wherein one cannot overlook the evidences of particular design and of appropriate purpose, it is inlaid between the fragments, wherever, when the limb comes to be used, the most new bone will be necessary for its strength. Thus, it may take the various forms of groins and buttresses, arches, beams, or bridges, according to the mode and degree of displacement of the fragments.

Thus placed, its development proceeds. I cannot tell the conditions which will determine, in each case, the route of development towards bone that the new material will take; nor whether the differences that may be observed are to be ascribed to the seat or nature of the injury, or to the condition of the patient. All these things have yet to be determined; and I believe that years of patient and well-directed investigation will be requisite for them. I can do little more than point out the modes in which the ossification may be accomplished.

And, first, it may be accomplished through perfect fibrous tissue. Thus I found it in a case of fracture of the lower part of the femur, and in a fracture of a radius; thus, too, I think, whatever new bone is formed after fractures of the skull, is developed; and thus, too, one may find, in the neighbourhood of fractures and other injuries of bone, ossifications of inter-osseous fibrous membranes, and of the tissue of the periosteum, or just external to it.

But, secondly, the bone may be formed by ossification of the fibrous tissue in a rudimental state. And this rudimental state may be that of either nucleated cells or nucleated blastema. Through nucleated cells, as the embryo-forms of fibrous tissue, bone is formed when granulations or inflammatory effusions ossify. The process may be often seen in the union of compound fractures, or of simple ones when much inflammation has been excited; but, best

of all, the ossification of nucleated cells, in granulations, may be observed when bone is formed in the mushroom-shaped mass of granulations that is protruded through the medullary canal of a bone sawn across in an amputation. In all these cases there appears to be a direct transformation into bone, without the intervention of either cartilage or perfect fibrous tissue.

The ossification of nucleated blastema, such as I have described as a rudimental form of fibrous tissue, may also be seen in simple fractures; and my impression is, that it is the ordinary mode of ossification in simple fractures of long bones that unite well and quickly. In such a case, in a fracture of the tibia, I found, in long-continued examinations, that the bone is formed without any intermediate state of cartilage; a finely and very closely granular osseous deposit taking place in the blastema, and gradually accumulating so as to form the delicate yet dense lamellæ of fine cancellous tissue. The nuclei of the blastema appeared to be enclosed in the new-forming bone, and I thought I could trace that they became the bone-corpuscles; but I could not be sure of this, nor, indeed, could I, in either this or any other examination, satisfy myself that the origin of these corpuscles is in any structures previously existing in the soft or unossified tissue. Their first appearance in the forming new bone is too obscure, I think, to warrant any positive opinion; neither could I trace how the nucleated blastema, or other structure which is included within the new cancelli, is transformed into the later and more perfect medulla; only, one sees fatty matter gradually accumulating.

Such are instances of the ossifications, for the repair of fractures, that may be accomplished through fibrous tissue; and all these are possible without the intervention of the smallest portion of cartilage.

But perfect cartilage, with its characteristic homogeneous intercellular substance, and its well-formed cells, and all the characters of the purest fœtal cartilage, may be produced. Through the ossification of such cartilage, Miescher and Voetsch and others, describe the repair of fractures as accomplished in dogs, pigeons, and other animals. I have not yet found the very same process in the human subject, but but I should think it would occur in favourable instances of simple fractures in children. In youths and adults I have found only varieties of fibrous cartilage; but these have presented numerous gradations of structure, from the fibrous towards the perfect cartilaginous structure. In different specimens, or sometimes even in different parts of the same, the reparative material may display—in one, fibrous tissue, with a few imbedded corpuscles, like the large nearly round nuclei of cartilage cells; in another, a less appearance of fibrous structure, with more abundant nucleated cells, having all the characters of true cartilage cells; and in a third, a yet more nearly perfect cartilage. Through any of these structures, and apparently by the same method through all, the reparative new bone may be formed. It may be formed, first, where the reparative material is in contact with the old bone, and thence ex-

tending, it may seem as if it grew from the old bone; or it may be formed in the new material in detached centres of ossification, from which it may extend through the intervening tissues, and connect itself with the old bone.

The new bone, through whatever mode it be turned, appears to acquire quickly its proper microscopic characters. Its corpuscles, being first of simple round or oval shape, and then becoming jagged at their edges, subsequently acquire their canals, which appear to be gradually hollowed out in the preformed bone as minute channels communicating with one or more of the corpuscles. The laminated canals for blood-vessels, I think, are later formed. At first, all the new bone forms a minutely cancellous structure, much like that of the fœtal bones in their first construction; but this gradually assimilates itself to the structure of the bones that it repairs, while its outer portions assume a compact laminated structure, and its inner or central portions acquire wider cancellous spaces and a more perfect medulla. But, in regard to many of these later changes in the bonds of union of fractures, there are so many varieties in adaptation to the peculiarities of the cases, that no general account of them can be rendered. Only, specimens and drawings, one and all, show the most striking evidences of design in the adaptation of the reparative process to the particular exigencies of each case; so that it might be said, with complete truth, of every instance, that whatever is necessary,—whatever may best, under the circumstances, repair the damage,—that is done.

Let me, in conclusion, remind you how this account of the ossification of fibrous tissue, in the repair of fractures, illustrates the most usual condition of fractures of which the repair is incomplete They are united by ligament—that is, by fibrous tissue: to the formation of this tissue the process goes on naturally, but at this point it is arrested: the fibrous tissue does not ossify, and its fibrous state, imperfect for the purpose of repair, is perpetuated.

Medical Gaeztte, July 20, 1849, *p.* 168.

[Mr. Paget gives the following as the conclusion to which he has arrived with regard to the *share which the inflammatory process takes in the repair of parts after injury*:]

1. That in the healing of a wound by immediate union, inflammation forms no necessary part of the process; rather, that its presence always hinders, and may completely prevent it. The healing by immediate union should be a simple re-joining of the severed parts, without the production of any new material; and in the same proportion as, in any case, inflammatory matter is effused, either in or between the wounded parts, in that proportion does the healing deviate from the true and best process of immediate union.

2. For subcutaneous wounds and injuries, as in divided tendons, simple fractures, and the like, nearly the same may be said. Inflammation is excited by the local injury, but its products form no necessary part of the material of repair; rather, the more abundant they are, the more acute the inflammation is, and the longer it con-

tinues, the less speedy and the less perfect is the process of repair. For here the necessary or best reparative material is a substance which, both in its origin and its development, declares its non-inflammatory nature; a substance which is produced without the signs of co-existent inflammation, and of which the development is different from that of the true inflammatory products. And this, which is most evident in the case of the healing of subcutaneous injuries by bonds of connection, is probably equally true in the case of subcutaneous granulations.

But 3dly. In the healing of a wound by primary adhesion, or by open granulations, we have evidence of a process of inflammation, not only in the presence of its ordinary signs in a degree generally proportioned to the severity and extent of the injury, but in the character and mode of development of the new materials that are formed for the repair. For these materials are, in all appearance identical with those of such effusions of lymph as all are agreed to consider the effects of inflammation.

Still, 4thly, in these cases we have evidences that the inflammatory process is necessary for no more than the production of the organizable matter,—and, in the case of granulations, for the production of only the first portions of it. The right formation of the cells, and, yet more evidently, their higher organization into cellular tissue and cartilage, ensue only while the signs of inflammation are absent. They are manifestly hindered or prevented when signs of inflammation are distinctly present, or when its existence may be suspected in consequence of the presence of some irritation, as a foreign body, dead bone, or the like. The continuance of suppuration during the process of healing is no proof of the continuance of inflammation, if the account that I have given of pus be true.

In these modes of healing, therefore, we may conclude that inflammation is only partially, and at one period requisite; and that, in regard to its requisite degree, the least amount with which an effusion of lymph is possible is that which is most favourable to repair.

Lastly, for the process of healing by scabbing, the absence of inflammation appears to be essential: indeed, the liability of our own tissues to the inflammatory process, and to the continued effusion that it produces, appears to be that which prevents their injuries from being healed as easily and surely, by the scabbing process, as nearly all open wounds are in animals.

Such may be regarded as the relations of the reparative process to that of inflammation, as it is commonly understood; but, I repeat, such a comparison can be made only for the sake of deference to the general state of opinion in matters of surgical pathology. In truth, we know less of inflammation than of the reparative process.

Medical Gazette, July 27, 1849, *p.* 138.

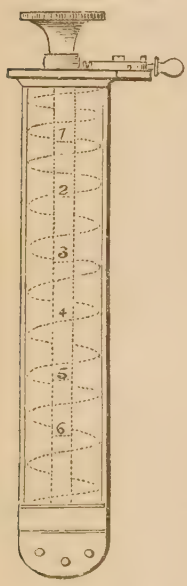
151.—*Description of a Galvanic Apparatus for applying Counter-Irritation.*—By Dr. T. Smith, late Physician to the Leeds Public Dispensary, and to the Cheltenham General Hospital.—[Dr. Smith recommends the following mode of using galvanism for the formation of an issue. It is a modification of that some time ago described by Dr. G. Bird. (See Retrospect, Vol. XVI., p. 351.) Dr. Smith says:]

A piece of perforated zinc is fastened or rivetted to a piece of platinized silver, or, what will do equally as well, and which I generally use, a sixpence, shilling, or half-crown, according to the size required. The apparatus thus prepared is to be applied with the zinc surface next to the body, the silver being uppermost; over this I place a piece of spongio-piline, previously moistened in salt and water, and retain the whole in close apposition to the skin by means of a few strips of adhesive plaster. At the expiration of every twelve hours, the battery should be removed and washed in salt and water, and then reapplied as before. At the end of twelve days, a deep white eschar is formed, from the action of the chloride of zinc. This may easily be detached, or allowed to slough out of itself, which generally happens about the fifteenth day, leaving a healthy looking sore. By reversing the galvanic apparatus, that is, applying the silvered surface to the wound, it will be found to heal up quickly; or it may be made to keep up a continuous discharge, by introducing split peas into the opening, or by dresssing it night and morning with savine cerate. Where, however, time is an object, and it is desirable quickly to induce counter-irritation, this may easily and speedily be effected by first removing the cuticle either by means of liquor ammoniæ fortissimus, or of acetum cantharidis, and then applying the battery to the denuded surface, and afterwards proceeding as in the former case. In this manner the same effects are produced in from four to six days, as would require twelve days by the other method. If the latter plan be adopted, it is not uncommon for the patient to complain of a gnawing pain in the part towards the evening of the third day, which, if not relieved, makes him restless and uncomfortable; a mild opiate, administered at bedtime, has generally had the effect of soothing the irritation, and preventing its future occurrence. In delicate females where it is desirable not to create more pain than is absolutely necessary for the induction of counter-irritation, the first plan is decidedly preferable. I have observed, on three occasions, where the idiosyncracy of the patient had previously rendered the exhibition of opium or its preparations inadmissible, that during the action of the battery they have produced the most tranquilizing effects. Acting upon this inference, would it not be well in such constitutions as are known to be susceptible to injurious impressions from a dose of any opiate, previously to irritate the cuticular surface by electricity or galvanism.

London Journal of Medicine, Sep. 1849, *p.* 826.

152.—*Account of a New Spring Syringe.*—By T. H. Wakley, Esq., Surgeon to the Royal Free Hospital.—[In order to meet the difficulties which patients, both male and female, experience in the use of the common syringe, Mr. Wakley has had some made with a spiral spring attached to the piston within the cylinder, and having a trigger and spring catch at the head of the instrument. Mr. W. says:]

The annexed engravings will furnish a more correct representation of the contrivance than any verbal explanation that I could offer. It will be seen that the shaft or rod of the piston is *graduated*, and that it thus enables either the surgeon or the patient to determine the exact quantity of injection which is used at one time.



The instrument being charged with fluid, and held with either hand, the *trigger* is at once placed at the disposal of a single finger. At the same time another finger, on being pressed against the side of the shaft or rod of the piston, can regulate the force which the spring shall exercise while discharging the contents of the syringe. The spring is set in action, and the discharge effected, by simply depressing the trigger. When the full force of the spring is required, pressure with a finger on the shaft of the piston must be avoided.

The spring syringe which I am using has been manufactured for me by Messrs. Spratt & Turner, of Titchbourne-street, and I have found it to be extremely useful and effective. The patients of both sexes, who have used it, speak strongly in favour of the facility with which the spring syringe may be employed. Females suffering under uterine maladies state that they can use it with the utmost ease, and with much advantage. One patient remarked that the spring was better than a "third hand." Any patient having two hands cannot experience the slightest difficulty, so far as the mere manual effort is concerned, in using the instrument.

The spring, in order to protect it from corrosion, may be made of platina wire; but steel spiral springs, as ordinarily constructed, will be found amply sufficient for all the usual purposes. The cylinder may consist of pewter or any other appropriate metal.

Spring syringes, of all sizes, and for all the occasions in which ordinary syringes are used, may be manufactured in the form and manner which have been described.

Lancet, June 23, 1849, *p.* 664

153.—*On the Connection of Muscular Action and Electricity.*—By Baron Humboldt.—At the last meeting of the Academy of Sciences, M. Arago read the following notes from the illustrious Humboldt, relative to the above interesting subject:—

"Neither the jeers of certain editors on German credulity, nor the negative results obtained by two of our first natural philosophers, have changed my convictions regarding the influence of muscular action on the movement and direction of the galvanic needle. We have recently repeated our experiments at M. Reymond's, and I invited M. Mitscherlich to attend, knowing his great dexterity in the management of delicate instruments. On giving tension to the muscles of the left arm, the needle was instantly made to move by M. Mitscherlich; and that in the direction predicted by M. Reymond, viz., one indicating a current from the hand to the shoulder of the arm which was in action. On stiffening his right arm, M. Mitscherlich made the needle move in an opposite direction, and traverse a smaller number of degrees; this arises from the fact, that the energy of muscular contraction is not always the same in both arms. Occupied, as I have been, for more than half a century with physiological researches of this kind, the discovery of M. Reymond has deeply interested me. It is a vital phenomenon, rendered sensible to us by an instrument of physics."

In connexion with the same subject I may mention the result of some curious experiments recently made by M. Ducros. The conductors of a galvanometer were applied, one to the forehead, the other to the neck. The needle remained steady and marked forty degrees. The patient's thigh was now strongly pinched, and under the influence of the pain, the needle passed to eighty degrees with great rapidity. The experiment, frequently repeated, gave the same results, and M. Ducros hence concludes "that all causes which increase vital activity react on the galvanic needle at a distance from the point of their immediate action."

Medical Times, July 14, 1849, *p.* 28.

154.—*Case of Bronchocele, cured by Seton.*—By Richard Hey, Esq., York.—[The patient was a young lady 21 years of age, who had suffered from bronchocele from early infancy. The tumour was of considerable size, of firm and resisting feel, and obstructed respiration to such a degree that surgical interference was deemed advisable. Accordingly, (the patient having been placed under the influence of ether), Mr. Hey tells us,]

I passed a skein of silk through one half of the gland, commencing at the upper part in the median line, and taking, as nearly as possible, a semicircular direction, and brought it out at the corresponding point below, thus embracing half the tumour, and from three quarters to one inch deep, so that the seton was fairly established in the substance of the gland. The patient did not appear conscious of any pain during the operation; and there was no hæmorrhage of the least consequence.

For the first five weeks after the introduction of the seton, no material change took place; the discharge was slight, and it was found necessary to use the savine ointment, which had the effect of increasing the discharge, which was healthy pus.

Some time in the course of the sixth week after the introduction of the seton, the patient began to suffer from a feverish attack, which was prevalent. On the third or fourth night after this fever commenced she was seized with a cough of the most distressing character, which harrassed her extremely. In consequence of being unable to expectorate, which had always been the case latterly when suffering from cough, the oppression was urgent, and the head soon became affected, apparently in the imperfect arterialization of the blood. The patient became delirious, and the tongue brown and dry. This condition continued about forty-eight hours, during which interval the seton had begun to discharge a thin dirty-looking, and most offensive matter, in great abundance, which in a short time relieved the irritation about the windpipe, and the delirium then very much subsided. This discharge from the seton continued most profuse for more than a fortnight, it being necessary to apply fresh dressings every two or three hours.

During the third week from the operation the discharge began to diminish, and towards the end of it, it seemed to loose its highly offensive smell, and to assume a more healthy character. There was never any appearance of sloughing, nor any indication of very active inflammation in the thyroid gland. The debility consequent upon this state of things was very great, but the convalescence, though prolonged, was ultimately complete.

As soon as the fever disappeared, and the discharge from the seton began to assume the appearance of healthy pus, the absorption or dissipation of the gland, which, as I have observed, had previously not been very marked, made rapid progress, and by the time the strength was re-established almost all vestige of enlargement had disappeared. I have allowed some time to elapse since writing the above, and am enabled to say that the cure is perfectly satisfactory, and the relief most grateful to the patient.

Remarks.—There is nothing new in this operation, yet I think it scarcely meets with that attention which it deserves—at least I have myself never before seen it resorted to. Any of your readers who may be interested in the matter will find several cases detailed in the *Transactions* of the Royal Medical and Chirurgical Society. Although the operation was known at least a century ago, yet Dr. Quadri, of Naples, seems to have revived it, and practised it to a

considerable extent about the years 1817 and 1818. Mr. Copland Hutchinson, in 1819, also tried it in some instances; Mr. T. A. Thompson and others have also adopted it, the general result of which seems to have been favourable. Mr. C. Hutchinson observes:

"Upon the whole the operation by seton in bronchocele is not, generally speaking, to be considered as dangerous, when performed by a judicious surgeon, well acquainted with the anatomical structure of parts, but in delicate and irritable habits, and in the hard lobulated species of the disease, some circumspection is to be observed; and, indeed, in such constitutions a cautious practitioner would well weigh the necessity of operating at all, unless the symptoms of suffocation and impeded deglutition became urgent." In some cases recorded, the danger from hæmorrhage seems to have been much increased from using a common seton needle. In order to escape such a contingency, I had a curved trochar made, and fixed in a handle, groved like a lithotomy staff, with an eye behind the shoulder; then, threading this with a single silk, to which a skein was attached, I passed the trochar in the manner already described, until I could get hold of the single skein; I then withdrew the trochar, and with great ease drew the skein through the canal thus formed.

Provincial Medical and Surgical Journal, Sept. 19, 1849, *p.* 516.

155.—*On the Treatment of Wounds of the Larynx or Trachea.*—By A. Eves, Esq., Surgeon to the Cheltenham General Hospital.—[It has been a subject of much discussion, whether, in cases of free transverse division of the larynx or trachea, we ought to bring the wound together by sutures, or to leave it open. If the wound is closed by sutures there is danger of asphyxia from accumulation of blood and mucus in the tubes; and if, on the other hand, the wound is not healed at an early period, the patient will seldom recover. Mr. Eves makes the following observations upon this point of treatment:]

First. What are our prospects in a case in which the air passages, being severely wounded, are, nevertheless, attempted to be healed by position, without sutures? It appears to me that such a case would almost necessarily terminate fatally, and such has been the fact in all the instances I have witnessed. The great mobility of the parts, in respiration and swallowing; the tendency of the lower part of the trachea or larynx to sink down, when completely divided; and the consequent irritation and congestion of the lungs; are the chief difficulties which we have to encounter.

Secondly. It has been remarked, that the patient in whom the larynx or trachea is divided, may die, if the wound be brought together by sutures, from accumulation of blood and mucus in the bronchial tubes and cells; but, as it is almost certain that the patient must die of these severe wounds, if sutures be not employed, it certainly follows that we ought to use them. Mr. M'Whinnie

says that sutures are not allowable in wounds of the air passages, that they are irritating and hurtful; but Mr. Stanley refers to a successful case by Rust, of Berlin, in which sutures were introduced through separated portions of the thyroid cartilage; and my friend, Mr. Rumsey, of Gloucester, at a meeting of the Gloucestershire Medical and Surgical Association, related a case in which the larynx was completely divided through the thyro-hyoid ligament, and in which he successfully introduced sutures through the thyroid cartilage. In this case the sutures did not produce irritation, and the case did well. But again the question recurs—Is there not danger in closing the wound? It must be confessed there is, and before such a practice is attempted, all hemorrhage must be carefully restrained. It is not my intention to give an account of all the means to be used in such cases, but merely to refer to a plan which, as I mentioned before, appears calculated to remove the ill effects of each kind of treatment to which I have alluded, at the commencement of these remarks.

The treatment, then, which I should adopt in a case in which the larynx or trachea has been freely divided, would be, after all hemorrhage was perfectly stopped, and after waiting a short period to be assured on that point, to bring the parts accurately together by sutures, introduced even through the cartilages, if necessary; then, if great difficulty of breathing should arise, so as to threaten immediate asphyxia, either from derangement of the rima glottidis, or from accumulation of fluid in the bronchial tubes and cells, I should immediately adopt the suggestion thrown out by Mr. Porter, of Dublin, and open the trachea by a longitudinal incision; of course, this must not be thought of, except there appears an absolute necessity for such a step; but it is satisfactory to know, that we have the means in our power of rendering that line of treatment, which is certainly the best, free from the dangers which have hitherto been apparently connected with it. I would also strongly insist upon the necessity of preventing a drop of nourishment from passing by the mouth for a few days, and the necessity of nourishing the patient by enemata. To prove the sufficiency of this mode of conveying nourishment, I would refer to a case treated in the Meath Hospital, in which, Mr. Porter states, the patient was thus supported for several months, positively refusing all food by the mouth.

Lancet, May 26, 1849, p. 557.

156.—*A new Vapour Bath.*—[At a meeting of the the London Medical Society,]

Dr. Golding Bird exhibited a vapour bath, similar in some respects to that of Duval, but easier of application, and cheaper; it is the invention of Mr. Moss, of Bartholomew-square, Old-street. It consists of an outer case or boiler, not unlike a waggon-boiler in appearance, and is supplied to a regulated height with water, for which purpose the feeding aperture is so constructed as to prevent mistake, the water finding its own level, and regulated in quantity

for the prevention of boiling over, or (as water) communicating with the pipes. A valve is attached for the safety of the apparatus. At the front end of the boiler an aperture, two inches in diameter, leads to an internal trough, which is water-tight, with the exception of an orifice or slot, open three quarters of an inch at the top, and extending nearly to the extremity of the boiler; this trough is designed for the admission of medicaments, &c. From the outer case, and into the interior of the trough, a perforated box and cover is fitted, which can be used or dispensed with at pleasure. Its design is for the reception of sponge or lint, upon which any liquid can be retained. The trough, box, &c., is covered by a small cover (not unlike the cover of a syrup bottle,) and can be adjusted instanter. In the centre of the cover a pipe is fixed, on which an elbow can be slipped to regulate any height or direction desired; to this can be attached a pipe, regulating the length for the conveyance of the vapour; the whole preparation occupying but a few seconds. The larger pipe, for general diffusion, does not require a pad, but a smaller pipe, suited for local application, is fitted with the perforated pipe and flannel pad. The vapour generated in the boiler passes through the trough, thence through the box onwards, medicated in its progress towards the pipe and the patient.

Remarkable States of the Muscular System.—Dr. Bird illustrated the case with which the apparatus could be applied, and he related three cases in which it had been found of great service in Guy's Hospital. A little boy, two years and a half old, a remarkably fine and healthy looking child, was brought to Guy's Hospital, about twelve months since, by his mother, doubled up, as it were, in her lap. His countenance was expressive of great anguish. It appeared that a few days before he had fallen into some cold water; the accident was followed by the usual symptoms of a cold, and by a total incapacity of bending the limbs, any attempt to extend them causing extreme agony. The case presented in this respect all the characters of acute rheumatism, but there was no swelling or redness of the joints, and only slight febrile symptoms presented themselves. The spine was healthy. The case was regarded as acute rheumatism. He was in the hospital for several days; perspiration often occurred, but the skin was cold and clammy. He was ordered the vapour bath, and after two or three applications of this remedy he could use his limbs, and in ten days was well. Two other cases of a similar nature had occurred, and had been treated in the same way with success. In two of these cases, the slightest *frottement* was heard during the progress of treatment; in one it had disappeared; in the other it still remained. In one lighter case, the iodide of potassium had been used with the warm bath; the patient got well, but was longer in recovering. These cases were peculiar, and could not be called rheumatic, in the strict sense of the word. There was no description of them to be found in books. In two of the three cases the urine had a milky appearance, and contained urate of ammonia.

Lancet, Oct. 27, 1849, *p.* 457.

157.—ON THE CHEMICAL CHARACTERS OF ANIMAL TISSUES.

By Dr. Aldridge, Dublin.

[In a paper on this subject, read before the Dublin Natural History Society, Dr. Aldridge stated that at a previous meeting of the Society,]

He had described the chemical means of distinguishing the principal tissues of which animals are composed; he had shown them that the ligamentary tissue, which is present in the so-called cellular tissue, as well as in the tendons, non-elastic ligaments, and fasciæ, is composed of interlaced fibres which dissolve in water, by prolonged boiling becoming converted into glue, and which swell and coalesce when subjected to the action of acetic acid; that elastic tissue differs in these respects from the ligamentary, whilst the fibrillæ of muscles differ from both by their behaviour with nitric acid and ammonia. In all these tissues the primitive cells rapidly develope in opposite directions into fibres; but the structure of cartilage is peculiar in the animal organism, resembling that of some of the simpler vegetables, in which numerous mother cells are scattered throughout a homogeneous and definite basis. Cartilage is also distinct in its chemical characters, being neither albuminous or gelatinous, but convertible, by ebullition, into the substance called chondrin. Out of the tissues enumerated, together with the nervous, all the organs of the higher animals are framed, so that it is a comparatively easy task to analyze the organic elements of any organ, by availing ourselves of the assistance which chemistry can afford to the microscope. But upon every surface of an animal, whether external or internal, there is constantly going on, as long as life continues, another process of organization distinct from that which forms the described tissues, but which is not inferior in importance. This exercise of nutrition consists in the perpetual construction of nucleated cells, that, unlike those which develope within the organs, have little or no tendency to elongate themselves into fibres. It has been said, that these cells are found upon every animal surface, both internally and externally; they cover the whole of the exterior, constituting cuticle, hairs, horn, scales, whalebone, tortoise shell, &c. Coating the mucous lining of the pulmonary intestinal, and genito-urinary canals, they are collectively called epithelium; and the same name is given to them when lining the ducts of glands, the interior of bloodvessels, or coating the free surfaces of serous cavities. If we examine the ultimate composition, or chemical properties, of these superficial cells, taken from different surfaces, we shall find slight differences between cuticle and horn, whalebone and hair, but these differences are not difficult to explain. They are due to the fact, that the substances we examine are not pure proximate organic principles, but are mixtures of various substances in different proportions. There is the cell-wall, which in the animal is probably, as it is known to be in the vegetable kingdom, a combina-

tion of proximate elements; then there are the cell-contents and the nucleus, and, in many instances, an encrusting matter on the cell-wall, which serves to bind the cells together. The consideration of these facts will serve to show the absurdity of bestowing distinctive chemical names, or recording ultimate analyses of substances so complicated and variable. But there are sufficient evidences to show, that all these cellular formations belong to the same group; and among these proofs, one of the most singular is. that a kind of albumen constitutes always a chief part of the cell-wall. This element can always be recognised by its behaviour with nitric acid and water of ammonia, treated with which it becomes a deep orange. Every one is familiar with the effect of nitric acid in staining the skin, and advantage is taken of this property in the dyeing of silk, which, on account of its albuminous origin, becomes of a deep yellow, when first immersed in nitric acid and subsequently in water of ammonia. Being composed of albumen, the production of these surface cells, whether cuticular or epithelial, is not difficult to understand; the albumen of exuded liquor sanguinis has but to coagulate for their production. On the papillary surface of the skin, we might suppose this coagulation to be effected by the acid perspiration, especially as we know that an analogous process is employed to coagulate albumen under other circumstances. Thus the spider's web is formed of threads which consist of albumen, coagulated by the acid secretion of the minute pore through which the secretion, previously liquid, is projected. Thefluid contents of the pollen, also, are coagulated by the acid secretion on the stigma of the flower. But there are other reasons for thinking that the coagulation of excreted albumen into the cuticular cell-wall is not due to the agency of acids. The similar epithelium is produced, for the most part, amidst an alkaline secretion; and it will be seen, further on, that it is more probable that the cell-wall determines, primarily, the acidity of the perspiration, than that the latter property is the cause of the formation of the cell-wall. Another character common to all cells, is that of chemically modifying the blood or nourishing juice; as soon as the liquid portion of the blood gets into a cell, so soon does it begin to undergo chemical changes. Each little cell may be regarded as a chemical laboratory destined to form a product necessary to the perfection of the organism. Each cell is a gland, and those organs that we are accustomed to call glands are only more or less complicated arrangements for extending capillary surfaces, upon which these cells can form themselves. Upon each surface, a different chemical change is effected in the cells; in some pigment is formed, as in the choroid in the youngest, and consequently least superficial cuticular layer, in negroes, supposed by some to be a distinct anatomical layer; and also in a similarly situated layer arranged in an areola around the nipple of pregnant women. Some cells convert the blood into mucus, others into bile, and others into urea, uric acid, and the various constituents of the urine. This chemical action of cells is co-extensive with their presence in both divisions of or-

ganized nature—in plants as well as in animals. The blood of plants, or the ascending sap, as it is called, is a saccharine solution:—to form cells in the vegetable, the sugar only suffers an isomeric modification; the cellulose or substance of which the cell-wall is composed does not differ in ultimate composition from sugar; but as soon as the cell is formed, it acts chemically on its contents, and causes the production of a new substance,—albumen. In a similar manner, the chlorophylle which colours the herbaceous parts of vegetables is at first formed in cells; but those cells like the epithelial, do not constitute any part of the permanent tissues, but are floating and evanescent; in time they burst, and permit the escape of their granular contents. It is thus, that the various kingdoms of nature shed light on each other's operations.

Dr. Aldridge continued to say, that in connexion with this subject there was a point to which he wished to draw attention. It might be perceived that both in the animal and vegetable kingdoms, the blood was capable of solidifying itself, and becoming organised into cells, without change in its chemical composition. In speaking of the blood, he of course alluded to the essential element of the nourishing juice; in one case sugar, in the other albumen; both these were capable of forming cells, the walls of which retained the chemical composition of the substance out of which they were made, although their properties more or less varied. There was this great difference, however, between the cells which have a saccharine, and those which have an albuminous origin. The saccharine cells constitute, by their aggregation, the fixed and permanent tissues of vegetables; the albuminous cells in animals, on the contrary, never enter into the formation of the permanent tissues. Those tissues may be gelatinous, chondrous, or fibrinous, but are never albuminous. The albuminous cells are the result of a normal hemorrhage from all surfaces; and from the very mode of their production, are destined to a constant and necessary exfoliation. None of the permanent tissues of animals have their chemical representatives in the blood. We do not find in that fluid gelatin, chondrin, or the material of muscle, for the latter is not identical with fibrin. It is not exactly correct to call the muscles, the cartilages, or the ligamentary tissues, permanent; but there is an obvious and essential difference between these elements of the animal frame work, and the abrasive cells which coat the surfaces of the organs. It would appear that the blood of a vegetable need only to have its elements arranged in a new manner, to fit it for building up the various organs; but the blood of an animal must undergo a chemical change to prepare it for that purpose. The laboratory character of albuminous cells is the most universal amongst them; and in this they resemble the cells which generate chlorophylle. This resemblance is the more remarkable, inasmuch as chlorophylle contains nitrogen, thus betraying its albuminous origin. Yet, although albuminous cells do not enter into the construction of the animal frame work, it is likely that all animal cells are originally albuminous. This is certainly the case in the ligamentary tissues, as is well exemplified

during the formation of false membranes in serous inflammation. In this case the albuminous fluid poured out into the serous cavity, organises itself into a multitude of albuminous cells, but these in time grow into ligamentary fibres. The latter become glue by ebullition; the cells from whence they sprung could never be converted into glue by the most prolonged boiling. This affords us a clue to a particular stage in the animal nutrition. The chemical preparation of the materials necessary for the growth of ligaments, tendons, areolar tissue, and, most probably, of cartilages and muscles, is confided to the laboratory function of albuminous cells. Henle supposes that ligamentary fibre proceeds from the development of the cell-wall, and that elastic fibre results from the development of the nucleus. But we can see, from the point of view to which we have attained, that it is neither the development of the cell-wall nor of the nucleus which produces the fibre; that the function of the cell is not merely to serve as a point of departure; but that it performs the part of a minute laboratory, in which is prepared the gelatine, the chondrin, or the muscular substance destined for the growth of the respective tissues.

From what had been said, the importance of these cellular formations in the animal kingdom would be manifest. They were the organs which prepared all the secretions; and which, moreover, produced those changes in the blood which fitted it for the purposes of nutrition. It had been said that all surfaces are clothed with these productions; but to this general statement the teeth, probably, constitute an exception. The blood-vessels are lined with epithelium, and the white and red corpuscles are analogous formations. The epithelium of mucous membranes is notoriously of three kinds; the pavement, the cylindrical, and the ciliated. The latter is covered with numerous minute processes like hairs, which constantly undergo an oscillatory motion when placed in a liquid. The members of the Society were familiar with the moving threads having club-shaped extremities, which are so constantly present in the spermatic fluid, and which have been so commonly considered as animalculæ, and named spermatozoa. Certainly nothing can be more difficult, whilst watching their motions, swimming about apparently under the influence of a guiding will, than to avoid the opinion of their distinctive animality. But there were various reasons which led him (Dr. A.) to disbelieve that these threads are animalculæ. In the first place, the nature of their motion, gradually diminishing in energy until it ceases, but never intermitting without some external mechanical cause. Secondly, their little tendency to putrify in comparison with any true animalculæ; and lastly, the extremely similar motions of the cilii of epithelium, even when broken off from the cells to which they had been attached. These considerations led him to think that these so-called spermatozoa are really analogous to ciliæ; and this opinion is much strengthened by the examination of their mode of origin, as delineated by Wagner. They are first observed as a bundle of threads, contained within a cell, those threads developed in length, become curved like an *u*,

and in time, by their elasticity, burst the cell which previously contained them, and then separate from each other. From this description, it would appear, that they differ from ordinary ciliæ, chiefly, in being produced within the cell-wall, instead of externally. It is probable that the same cells elaborate the fluid sperm, a chemical principle of a very peculiar nature. There is a difference worthy of note between the products of the cells coating external and internal surfaces. The cutaneous secretions are usually acid; the secretions from the mucous membranes are for the most part alkaline. There are apparent, rather than real, exceptions to this rule, such as the gastric juice, the urine, &c.; but generally the fact is as stated; and this is connected with a difference of electrical distribution, so that Matteuci, upon the introduction of a galvanometer, as an element of a circle, of which a mucous surface and the skin also constituted members, was enabled to produce a divergence of several degrees. Dr. Aldridge believes that this difference is owing to the relative facility of contact with atmospheric air. It has been already seen that cells are little chemical apparatuses, for changing the composition of the blood-elements; but that the aid of extraneous agents is frequently resorted to in accomplishing these metamorphoses is no less evident. Thus it is in the leaves and bark of plants, parts most exposed to the air, that the proper juices are formed. In warm blooded animals, the iron of the food is combined in the capillaries with the first products of waste, to form the colouring matter of the red globules. Benzoic acid, when absorbed into the circulation, combines with some of the elements of the urine, to produce hippuric acid. It must be admitted, therefore, that the free access to air is a cause most likely to modify the nature of secretion. Now, there is reason to believe that sugar and ammonia are among the primary products of the decomposition of albumen. It is not improbable that these compounds are common to all animal secretions. When sugar is in excess in the urine, it constitutes the disease named diabetes mellitus. A variety of sugar is kown to be a constant constituent of milk. Sugar has latterly been detected as a very common constituent of bile and blood. Dr. Aldridge himself has lately shown that it is always present in the white of eggs; and in incubated eggs, sugar, identical with that of milk, has been detected. Now, if we suppose sugar to be a constant product of the laboratory action of albuminous cells, we shall have to admit its presence both in the cutaneous and mucous secretions. But in the former case it will be secreted in free contact with air; in the latter case it will be more or less protected from the atmosphere. It is not wonderful, therefore, that the sugar secreted by the cuticular cells should rapidly become converted into acetic or lactic acids, or that the sugar secreted by the epithelium should be slow to undergo this change. Both secretions contain ammonia, which can readily be detected in them; but this gas escapes from the skin, while it is retained in the mucous secretion. These considerations explain the differences which exist between the two classes of secretion in relation to their chemical re-actions.

It is not, however, on surfaces only that albuminous cells exercise their important functions. It has been seen that they are the originators of all the tissues, preparing within their cavities the pabulum for the growth of each. It would appear, also, that they exert a considerable influence in determining the structure of organs. Thus the ducts of glands have usually been described as evolutions of the lining membranes of neighbouring mucous or cutaneous surfaces. But this observation does not appear to be borne out by the examination of the simple follicles of the intestines. Many of these will be found to be furnished with excretory orifices; but in some, none such can be detected. If the latter be regarded as a preliminary step in their formation, the production of a simple or conglomerate gland would seem to depend on what might be likened to one or many little abscesses, which, bursting on the nearest and most accessible surfaces, would produce a simple or ramified duct. This is what is known to take place in the ovary upon the escape of the ova; but, in that case, the termination of the fistula of escape soon becomes cicatrised, constituting the *corpus luteum*.

Medical Times, May 26, 1849, *p.* 587.

158.—ON THE NON-IDENTITY OF MAGNETISM AND ELECTRICITY, AND ON SOME POINTS CONNECTED WITH WINDS.

By J C. Atkinson, Esq., Westminster.

[Mr. Atkinson thinks that magnetism and electricity are not identical, and differ in the effects which they produce upon the weather and climate. He says:]

Magnetism of the earth is a state of continual and restless fluctuation, and its changes from moment to moment are strictly simultaneous at every point where observations of this nature have been made. It is my impression that cold is an effect of magnetic influence, as solar heat in the atmosphere is an effect of electric action, and I conclude, that the vital principle of temperature is the result of the combined agency of these two forces, variously modified, according to seasons and latitudes of the several places. The range of the barometer gradually increases towards the North Pole. The rise of the mercury may be observed in this country during the prevalence of northern winds, conveying as they do, magnetic currents, till at last it reaches to two or three inches. On the contrary, between the tropics, the variations of the barometer are exceedingly small; while winds from the southward in temperate latitudes, if the electric current or wind is continued for any time, will cause a tendency in the mercury to fall.

The winds are more powerful in the production of weather than would appear at first sight; and although the moon was always supposed to exercise great control over the weather, yet it is strange

that both Drs. Herschel and Clarke, great observers of the heavens, state, in the weather tables,—as a set off, I conjecture, to her influence,—"If the particular change of the moon, full or new, takes place when the wind is so and so, it will be fine for the ensuing week, and the reverse if it is not." I ask, does this prove the dominion of the moon over the winds, or the contrary?

Magnetism.	*Electricity.*
1. The intensity of the magnetic force in different parts of the earth is according to the distance from the poles.	1. The intensity of electric force is greatest at the tropics, and diminishes as it approaches the poles.
2. The frigid zone, where magnetism is in the greatest intensity, enjoys an atmospheric calm which is unknown in temperate regions; it has no storms, no hail, and scarcely a tempest.	2. The most vivid lightning and the loudest thunder are peculiar to the tropics: here vegetation is vigorous and active at all seasons.
3. The splendour of the aurora borealis, reflected by the snow, dispels the darkness of the polar night, but is not accompanied with danger.	3. Igneous and fiery meteors, fire-balls, lightning, are the peculiar electrical phenomena of the tropics.
4. Magnetism suspends the decomposition of vegetable and animal matter.	4. Electricity assists decomposition in all vegetable and animal substances.
5. In northern latitudes the mercury of the barometer always stands high: in the temperate latitudes it is liable to frequent variations.	5. The mercury of the barometer is always low in the torrid zone, and varies but little during the changes of weather.
6. Magnetism has been known to produce sedative action on the human frame, when applied under certain conditions, causing repose, if not sleep.	6. Electricity is of an exciting character, increasing the circulation of the blood, and rousing paralytic functions of the nervous centres.
7. The northern currents of atmosphere, accompanied as they are with magnetism, have the power of expansive action on the mercurial column of the barometer, wherever it may be placed, either in a warm room or in the open air.	7. The southern currents of the atmosphere, accompanied with free electricity, cause a fall of the mercurial column, under any circumstance of position of the barometer.
8. Snow from the north-west wind has been found to contain more oxygen than rain or river water, and more carbon.	8. Electricity causes the rain to descend with less oxygen than snow water, and less carbon.
9. The magnetic intensity of the earth is indicated by needles	9. In sailing towards the equator, with a magnetic needle

suspended vertically; and in sailing from England towards the North Pole, it is seen that the needle dips, or inclines more and more with the increase of latitude, until, at a certain point, it remains exactly perpendicular, with its south pole downwards. suspended in the like manner, the inclination or dip gradually decreases, until the needle rests in a perfectly horizontal position.

The above are some of the reasons which have induced me to think that magnetism and electricity are not strictly identical, their operations being as distinct as heat and cold; and I have therefore come to an analysis of the influence of the winds, and a theory to establish something of a definite character. Winds are not decisive as to their influence on health, when continually changing; that is, not remaining in any fixed quarter for two or three hours. Winds have been known to blow from different currents, as spoken of before. Mr. Green, the aeronaut, in his celebrated aerial trip from Vauxhall Gardens to Nassau, in Germany, frequently availed himself of the different currents of air at different altitudes, by raising and lowering his balloon; and in this way he regulated his passage across the Channel, opposite Dover. The highest elevation attained by the aeronaut, during his voyage of nearly 500 miles, (British) was 12,000 feet above the level of the sea. At the highest point of elevation, it appears, from the account, that there was less circulation of air than at any point below it.

On the earth's surface, then, we have frequent changes of wind, and this more particularly in temperate latitudes or climates; and hence there are frequent alterations in the atmosphere as regards both temperature and electro-magnetism. Sometimes a wind well known for its peculiar mild character assumes so different a nature as to be mistaken for a wind from altogether another quarter of the globe. This arises from various winds supplying one current of air. For instance, we have a south-west wind of some velocity, of a stormy nature, and highly charged with electricity. Suppose, then, this wind to have a parallel breadth of any number of miles, and then to be supplied by north-east and north-west winds, of course, under such an influence, we shall have a great reduction of the temperature, and other qualities somewhat reversed, for a time, at least.

A noth-east wind, on the other hand, will frequently have its real nature or character altered, owing to rapid changes of aerial currents during stormy or thundery weather; and this happens more particularly when the observer is situated on the external edge, as it were, of a parallel current of wind, (which varies in diameter, at various seasons, from ten to one hundred miles;) and hence an unusual temperature is given to it. In the centre, however, of the said parallel current you have the true quality unmodified.

We now come to the question of health and disease under the operation of the several winds. The north wind (I here refer to

the magnetic north) has a different influence on different constitutions and ages. The temperature of this wind, of course varies, as has been before intimated, according to the seasons; but its general characteristics are always alike. This wind is usually unattended by hail, or even snow,—although, a few points to the eastward, it will often be followed, in the winter season, by a supply of the above phenomena.

Lancet, July 28, 1849, *p.* 91.

159.—*Treatment of the Sting of a Bee.*—The immediate application of honey is good; the use of indigo is better; and tobacco-juice, though we cannot speak of it from experience as of the other two, may be equally or more efficacious. It is thus recommended in De Vriend van Den Landman:—"Apply the juice of tobacco as you find it in the mouth-end of a smoked cigar, or in the reservoir of a German pipe. It not only immediately relieves the pain, but prevents swelling." The substance recommended is not, it must be remarked, the juice, but the empyreumatic oil; which, as the experiments of Dr. Morris (Edin. Med. and Surgical Journal) have shown, is a much more energetic poison than the juice.

London Journal of Medicine, Nov. 1849, *p.* 1061.

160.—*New Mode of Ventilating by the Syphon.*—[The following is copied into the Lancet from the Literary Gazette:]

Dr. Chowne has enrolled a patent for Improvements in Ventilating Rooms and Apartments, of the perfect efficacy of which, we believe, there cannot be a doubt, and on a principle at once most simple and unexpected. Without going into details at present, we may state that the improvements are based upon an action in the inverted syphon which had not previously attracted the notice of any experimenter—viz., that if fixed with legs of unequal length, the air rushes down into the shorter leg, and circulates up, and discharges itself from the longer leg. It is easy to see how readily this can be applied to any chamber, in order to purify its atmosphere. Let the orifice of the shorter leg be disposed where it can receive the current, and lead it into the chimney (in mines, into the shaft,) so as to convert that chimney or shaft into the longer leg, and you have at once the circulation complete. A similar air syphon can be employed in ships, and the lowest holds, where disease is generated in the close berths of the crowded seamen, be rendered as fresh as the upper decks. The curiosity of this discovery is, that air in a syphon reverses the action of water, or other liquid, which enters and descends or moves down in the longer leg, and rises up in the shorter leg! This is now a demonstrable fact; but how is the principle to be accounted for? It puzzles our philosophy. That air in the bent tube is not to the surrounding atmosphere as water, or any heavier body, is evident; and it must be

from this relation that the updraft in the longer leg is caused, and the constant circulation and withdrawal of polluted gases carried on. But be this as it may, one thing is certain—that a more useful and important discovery has never been made for the comfort and health of civilized man. We see no end to its application. There is not a sanitary measure suggested to which it may not form a most beneficial adjunct. There is not a hovel, a cellar, a crypt, or a black close hole any where, that it may not cleanse and disinfect. We trust that no time will be lost in bringing it to the public test on a large scale, and we foresee no impediment to its being immediately and universally adopted for the public weal. We ought to remark that fires or heating apparatus are not at all necessary: and that, as the specification expresses it, "this action is not prevented by making the shorter leg hot while the longer leg remains cold, and no artificial heat is necessary to the longer leg of the air-syphon to cause this action to take place."

[Extraordinary as this may appear, we have witnessed the experiments made in various ways, with tubes from less than an inch to nearly a foot in diameter, and we can vouch for the fact being perfectly demonstrated. Light gas does descend the shorter leg when heated, and ascend the longer leg where the column of air is much colder and heavier!—Ed. *L. G.*]

Lancet, Sept. 15, 1849, *p.* 305.

161.—ON THE MODUS OPERANDI OF TONIC REMEDIES.

By Dr. Golding Bird, F.R.S

[Dr. Golding Bird brought forward the following views of the modus operandi of tonic medicines, in a lecture delivered before the College of Physicians. After referring to the recent writings of Dumas and Barthe, as throwing some light on the mode in which medicines act, Dr. Bird said:]

If I were to point to any one class of therapeutical agents about which there is less satisfaction among professional men, (with the exception of resolvents,) I should pick out the class called tonic. According to the general signification, almost anything might become tonic—it merely expressing a generic term; and, if you look at the various drugs comprehended under this head, it is curious to observe how many of them are totally distinct,—thus: iron, bismuth, silver, sarsaparilla, gentiana, and serpentaria. Now, I select these as specimens all included under the generic term—tonics, and yet how different their modes of action, and how many practical mistakes occur in their administration. Any particular remedy being *called* a tonic, the patient believes a tonic has been given. Thus, the mucous membrane of the ilium may be weak, and because iron is a tonic, therefore iron is given. How often has the practitioner seen cases where the strength is deficient, and where the patient has been treated with bitter tonics without any benefit

resulting. Much of this failure occurs through want of a due consideration of the particular condition which makes it necessary to give tone to the system. We may be said to have all a series of forces acting within us; these we do not seek to define; but if we could trace disease to their undue action, we should be better prepared to administer our remedies. Now, my object is, to endeavour to throw some light on this matter. If we admit the existence of a vital force, we shall regard the several forces in a different light than those operating on dead matter; in the latter case, there are a series of forces following definite laws, and we can accurately point out their results, and apply certain mathematical laws to them. But not so with respect to the living being; here the force cannot, at present at least, be rendered obedient to certain laws so readily. Therefore, the necessity of studying these forces. All will admit the existence in the body of two great forces,—

1. The vital force – as opposed to
2. The chemical.

While we admit the first of these, it is obvious, that, unless some cause existed in the body which tended to break up and emaciate, in proportion as we took food, so we should grow on to any size and strength; and therefore it is necessary, at the same time, to admit a chemical force operating to waste. These forces are antagonistic, we know; but they modify each other.

But there is another series of forces which has been pointed out by Barthe and Dumas. In investigating the influence of remedies, these writers indicate the existence of two distinct forces in every organ, and which influence its growth. Thus the radical (or vegetative) force is inherent to it, and its influence is to induce every organ to carry on its particular function. This is a passive force, or organic inertia. Then there is an active force influencing any particular organ, exciting, diminishing, or modifying its function; and the sum of these two constitutes the vital force. Thus, suppose we have a voltaic battery, and the wires be plunged in some electric compound, capable of conducting electricity, the vessels containing the fluid represent the kidneys; the voltaic battery represents the series of organs which developes the principle of life, so far as the nerves are concerned. The battery being ready, but uncharged, is in a passive state; but on filling it with acid, an electric current is induced, which traverses the wires, and produces decomposition; but it also points out the polarised condition of the particles. Now, the current from the battery would represent the active force, and the polarised condition of the particles the radical. It has been thought, that the radical force is excited by the acting force of the battery, and that that action remains, although not in the battery —a polarised condition which keeps up an action, and thus produces a tendency for composition to go on. Then, using this rude analogy, let us suppose a dead body, and that we had the power of lighting up the influence of life. While it was inactive, secretion would not go on; but could we breathe into it the breath of life, what would it do? –at once set up the action of the system, and

that force —the nervous influence—would at once be determined to certain organs, and these would resume their function. Now, that there exist these two distinct forces, we have no doubt, for we may control the one without affecting the other A patient is dropsical; the kidneys do not act; a stimulus is applied, and they perform their function;—the fact is, you have determined the acting force, and thus quickened secretion. On the other hand, you have a patient with gout; the urine is very scanty,—there is hardly any secretion at all, and yet the acting forces are too active, if possible. Here, by giving alkaline salts, what do we do? We do not determine more force to the kidneys, but increase the secretion of urine. As another illustration, we may say, that the acting force is like the influence of steam in the steam-engine, while the radical force acts like the fly-wheel. Then it becomes a question—How far we can act on these forces? Take the radical force, for instance. Its intensity differs much: of two individuals, they may have very different degrees of radical forces, and yet be apparently healthy; the one was born of healthy parents, and living in the open country; this one would live long, secrete properly, and live healthy; but another acquires struma. What is the result? We find that his glands secrete unhealthy matter, and then we get the horrors of scrofula. Now, the combined influence of these two forces developes the vital force, or that which is equal to the radical force plus an active force. The combined influence of these determines the healthy state of the patient—*quoad* constitutional health and vigour. Hippocrates distinguished this condition as one beyond which health could not be carried, and the writers above mentioned particularly point out, that there is a point in stimulating these forces, which cannot be exceeded without affecting the health. Thus, if you stimulate a man in fair health, and give him iron as a tonic, if that absorbes, you increase the blood, and he becomes apoplectic or plethoric, with a tendency to hæmorrhage; or if you give a bitter tonic, instead of his becoming more vigorous, you excite the circulation, and produce headache and distress. These writers suppose that Hippocrates alludes to the vital force when he speaks thus in his third Aphorism: —"An extremely full habit of body, in those who devote themselves to gymnastic exercises, is dangerous; for as it can neither remain in the same state, nor change it into a better, it must necessarily become worse. For this reason, it is advisable that the too full habit be reduced, in order that the body may commence a new course of nutrition. But this reduction must not be carried to an extreme, for that would be dangerous; it must be done only as far as the nature of the patient will admit."

Now, these two forces, the radical and the active force, are active in maintaining the status of the body. But there are others, also, which are active in enabling the body to undergo repair. One of the most mysterious of these is the force of assimilation: it is this which has been so erroneously considered as synonymous with digestion. It is not so, however, but widely different. It is a force lighted up by the vital principle, and which enables the individual

to cause the pabulum to be assimilated to his own structure. Thus, even in the vegetable world, we have in the same hedge, belladonna, nightshade, hemlock, the blackberry, and the rosa canina; and we find each one of these absorbing the same fluid from the earth, and yet giving out qualities totally distinct; for instance, we have the hemlock and the nightshade developing deadly poison, and the rose and the blackberry giving out citric acid and saccharine matter. This is one of the most remarkable phenomena in nature, and in the human body we see it to perfection; and one of the most important weapons is that which thus helps the physician to control disease. Then we have another—a certain force of vital resistance, which appears to be active in preserving existence and in opposing changes; this is synonymous with the vital force. Taking the views of the two authors, these forces differ to a curious extent in different persons. In some the force of resistance is very prominent; in others that of assimilation is very greatly developed. Thus a person may be to all appearance healthy, florid complexion, and stout; yet he will suffer intensely from heat, he will get chilblains from exposure to cold—he will have fits on bleeding him to one ounce; such a one has very little of the force of resistance, and we also know how distressing it is to treat patients of this character. On the other hand, we may meet with one whose force of assimilation is very small, but where that of resistance is most remarkable. Look at these skeletons, who go through their work, hardly assimilating any of their food, and yet stand all the wear and tear. These are they who go through, under all sorts of influences, and never seem to wear out. Now, this force of resistance is one which we may excite and develope, and one which we may with equal facility crush. A person under the influence of plague, or of typhus poison,—he is at once crushed by it,—chemical decomposition going on even while he is living,—being a mass of bed-sores, and he dies in the most frightful state: here the force of vital resistance is destroyed suddenly. In others this force gradually sinks, as for instance in those under the influence of marsh miasma; here the force is gradually diminished, till at last, on a poison be received, they are crushed, and die immediately, without any particular function being interfered with.

But, after all, what are these forces. Trace them to their one great source, and, undoubtedly, if we look to any part of the nervous system, we should look to the ganglionic system for the origin of them. Admitting, then, the existence of these forces, it is obvious that the definition of "tonic" will admit of a little clearing up. By "tonic," we mean anything which will increase energy. Included under this term are many remedies which widely differ, as arsenic and gentian. But in isolating the conditions in which tonic remedies do good, I shall distinguish them by putting them into two great classes,—the neuro-sthenic or dynamic, that which gives power; and the analeptic or hœmatic, which invigorates by restoring healthy constituents to the blood. Admitting, then, the existence of the forces, and that we have a set of remedies that will

increase the vigour of the acting force, and give tone and vigour to the blood, we thus break ground; and, in looking to the class which I call dynamic I would point out the results of my own observation in reference to the action of these remedies. It appears to me that they may be divided into classes distinct from each other,—a division which I have found of very great service to me; if it answer no other purpose it assists a short memory, and I confess mine is of that character. Taking, then, the neuro-sthenic, those which increase the powers of the body, I venture to divide as follows:—

NEURO-STHENIC—(DYNAMIC.)

1. SIMPLE—
   - Quassia.
   - Conduus.
   - Chiryata.
   - Erythrea.
   - Menyanthis.
   - Calumba.

2. CALMATIVE—
   - Gentiana.
   - Humulus.

3. NUTRIENT—
   - Tussilago.
   - Cetraria.
   - Sarsa.
   - Hemidesmus.

4. ANTIPERIODIC—
   - Quina.
   - Cinchonia.
   - Narcotin.
   - Salicin.
   - Beeberin.
   - Æsculin.
   - Arsenic?

5. NERVIN—
   - Shydmia.
   - Zinc.
   - Silver.
   - Bismuth.
   - Copper.

6. STIMULANT—

Stomach.
- Absinthium.
- Anthemis.
- Aurantium.
- Cascarilla.
- Myrrha.

Skin.
- Serpentaria.
- Contrayerva.
- Acorus.
- Guaco.
- Cusparia.

Intestines
- Simaruba.
- Hæmatoxylon.

ANALEPTIC—(HÆMATIC.)

- Iron.
- Food.
- Oxygen.

From this, I think, we may see our way into the mode in which these act, according to the view which we have taken of the several forces. We talk of bitter tonics as adding to the vital powers; but, in so administering them, we only acquire the power of applying a force in a convenient, economical way. If by the whole amount of my physical force I cannot lift this piece of chalk, no human power will enable me to do it without the assistance of a mechanical force. If the chalk is such that I can lift it

one foot in a second, no human force will enable me to do more; but, by applying a lever I could lift indeed higher, but, then, instead of lifting it one foot in a second, it would take ten seconds; so that, as to physical force, we cannot generate it. So here, and with regard to bitter tonics classed under the head "simple,"—to which I shall only allude in this lecture; they possess a crystallised bitter principle, which in all of them is analogous. These peculiar principles are incapable of exerting any particular influence on the secreting organs; but they appear to influence the stomach by increasing its assimilative force. This is the feature of this class of tonics; and I believe they do it by their influence on the vegetative nerves of the stomach, and by causing an increased secretion of gastric juice. In these (the simple) we have a different form of bitter tonics, consisting in the absence of a stringent influence They owe their value to their crystallised principle, with the exception of the last two, which have not been examined; and when taken, they increase the assimilative force of the stomach—an all-important function, because it is the initiatory stage from which the formation of healthy blood is derived. Others of these tonics differ in the addition of other influences. The name of the second class is applied to a certain series of dynamics, which, while they increase the assimilative force of the stomach, do more; they act on the extremities of the nerves of the stomach, allay the debility of the organ, and act as sedative agents. The first is the hop; the second is not so generally admitted, but that it exists is certain. If the first is distilled, a certain oil is obtained like opium, and we sometimes meet with persons who cannot bear gentian; so that in the second class, I isolate one in which the remedies differ from the first in acting as sedatives and thus add another influence to another series of nerves.

Medical Times, July 14, 1849, *p.* 24.

162.—*Treatment of Dropsy by Colchicum.*—By M. Edouard Vandezande.—The preparations of *colchicum autumnale* are usually employed solely on account of their efficacy in rheumatism and gout. Störck, however, who introduced this plant into the materia medica in 1763, recognised it as a powerful hydragogue, and employed it as a succedaneum to squills. In spite of his recommendations, and of the favourable reports given by Plenck, Quarin, Zacht, Cullen, and others, the employment of colchicum in dropsy has not extended, and at last has been almost entirely neglected. In later times, the attention of some physicians has been fixed afresh on the power which Störk recognised in colchicum. Among others, Dr. Kennedy, in a memoir presented to the Surgical Society of Ireland, relates that he has, in a large number of dropsical cases, obtained astonishing results from the vinous tincture of this plant. Having also made use of this preparation in some cases of serous infiltration, and accumulation, I have thought it not unprofitable to give a summary account of the results which I have obtained; first, because they are of a nature to lead my professional brethren to have recourse to the hydragogue power of the plant in analogous cases;

and also because the effects of colchicum have not always been so complete or so manifest as in the cases which I have observed, of which I shall record the principal.

Case 1. Jean Bulckaert, a workman, aged 42, of weak constitution, was seized, three years ago, with intermittent fever in Flanders, where, as is well known, this disease is endemic. Having been under treatment for several months in the hospital at Dixmude, he recovered; but, on his returning home, where he could only procure insufficient food, the disease was not long in recurring. Instead of then applying for medical advice, he had recourse to some domestic remedies, which produced no effect. The patient soon presented all the characters of paludal cachexia. The lower extremities, and the whole body in succession, became œdematous; the abdomen was distended, and the respiration impeded. It was when in this condition that Bulckaert first applied to me. After febrifuge tonics and diuretics had been used for six weeks, the œdema disappeared, and the state of the patient's health seemed satisfactory, although there still remained a slight enlargement of the spleen, of which he neglected to get relieved. Last April, two years after the first appearance of the fever, I was again sent for by him. The dropsy had reappeared, but this time it had not been preceded by an attack of fever: there was excessive serous infiltration of all parts of the body, and the urine was slightly albuminous. The same treatment as before was adopted, but without the least success; I then prescribed the wine of colchicum in daily doses of six grammes (about ʒ ii.), increasing the dose by a gramme daily. The medicine was well borne by the patient; it was increased to 40 grammes daily; this quantity was continued for four days, at the end of which time the patient was free from dropsy. During the latter period of the treatment, the alvine evacuations were abundant, and the quantity of urine was considerably increased.

Case 2. A woman, named Messiaen, became dropsical after an attack of confluent small-pox; the dropsy was preceded, for some days, by paroxysms of intermittent fever. Neither nitric acid nor heat showed the presence of albumen in the urine. I treated her with wine of colchicum, in doses of eight grammes, without any other medicine. At the end of a fortnight the anasarca had disappeared, and the patient felt herself completely restored.

Case 3. A poor woman, who had twice been attacked with articular rheumatism, from which had resulted an organic affection of the heart, with habitual dyspnœa, observed her lower extremities to become œdematous, without any known cause. The extreme misery in which this woman was, and her dirty habits, gave but little hope of a prompt result from any treatment whatever. I resolved, however, to give her colchicum, especially as I thought that the rheumatic diathesis was connected with the production of the dropsy. The medicine was not well borne, in doses of eight grammes. I diminished the dose by one half; but in a few days, I had to discontinue its use entirely, in consequence of the occurrence of vomiting, dry tongue, and intense thirst. Some time after, when

the symptoms of gastric irritation had been removed, I again employed the colchicum in smaller doses, but without any effect. Mild tonics were then prescribed; and, under their use, the œdema at length disappeared. The woman has still œdema of the feet.

Case 4. A miller, addicted to the use of alcoholic liquors, had for some days complained of *courbature*, loss of appetite, and a remarkable diminution of urine, when he perceived symptoms of dropsy. I first saw him on the 15th of last May. The lower extremities, the scrotum, and the hands, were very œdematous: the face was much puffed up; the abdomen was hard and distended; the respiration was impeded. On testing the urine with heat and nitric acid, a large quantity of albumen was detected. The tongue was furred, the stools unfrequent, and the pulse somewhat febrile. The patient complained of no pain in any part of his body. With the object of cleansing the digestive canal, I prescribed an emeto-cathartic, consisting of a deci-gramme ($1\frac{1}{2}$ grain) of tartar emetic, 32 grammes of magnesia, and 250 grammes of water, of which a wine glass full was to be taken every half hour. The patient had several evacuations from the stomach and intestines, without any marked effect on the dropsy. On the 18th of May, wine of colchicum was ordered in doses of six grammes, and was very easily supported. The next day, I increased the quantity by two grammes, and continued it for ten days. As this treatment produced no amelioration of the symptoms, the patient importuned me to try some other means. The urine being still albuminous, I had recourse to the internal administration of nitric acid, as recommended by Professor Forget. This medicine had the desired effect. In four or five days, there was a remarkable diminution in the size of the scrotum, while the œdema of the extremities decreased; the abdomen became softer, and the face less puffy; the urine was more abundant and less albuminous. In spite of the continued use of the medicine, the patient remained in the same condition for some time; and it was only after using Bestuchef's tincture* for some days, that the œdema and cachexia disappeared, to be succeeded by rapid convalescence, and soon by recovery.

These observations appear to me to be of a nature to induce my professional brethren to have more frequent recourse to the use of wine of colchicum, in the treatment of certain dropsical affections. I am persuaded, though unable to specify the cases in which its employment is specially indicated, that this medicine may often be advantageously substituted for the hydragogues in most common use. At all events, I would not recommend it in all cases of albuminous nephritis; for there are medicines which act more directly on this disease, and, which, therefore, seem to me to be preferable.—*Annales de la Soc. d'Emul. de la Flandre Occidentale*, as quoted in the *Revue Medico-Chirurgicale de Paris* for August, 1849.

London Journal of Medicine, Nov. 1849, *p.* 1039.

* Bestuchef's tincture, otherwise called Klaproth's tincture, or ethereal tincture of chloride of iron, is composed of dry perchloride of iron, one part; Hoffmann's liquor, seven parts.

163.—*On Oil of Anda, a new Purgative.*—By Alexander Ure, Esq.—[The Anda Gomesii is a Brazilian plant, belonging to the class and order Monœcia Monadelphia, N.O. Euphorbiaceæ. Its seeds are used as a cathartic by the inhabitants of the Brazils, and the oil expressed from them has been used in North America. Mr. Ure having administered this oil in several cases, gives the following account of its effects. He says:]

It may be observed, that the average dose of oil of anda administered was twenty drops, and to secure its entrance into the stomach it was swallowed on sugar. It offered nothing unpleasant to the taste, produced none of that heat in the throat which croton oil creates, seldom occasioned nausea or griping; it rarely operated within a period of two hours, although in one or two instances I have known it act within half an hour after its ingestion.

Towards the maintenance of health it may be laid down as an essential condition, that the *effete* remains of food, and of the spent tissues and secretions present in the alimentary canal, should be evacuated at periodical intervals. Human fæces, of which these may be said to form the aggregate, exhibit all the signs of putrefying matter; they have a putrid smell, and animalcules of the *infusoria* class are developed in them. Hence it may be readily supposed, that if the fæcal excrementitious matter, averaging five ounces daily, be allowed to accumulate and remain for an undue length of time in the body, it must become a source of noisomeness and disease. It is, indeed, well known that some persons, who have passed several days without being thus relieved, exhale a stercoraceous odour. One of the worst forms of putrid fever I ever witnessed, and which eventually proved fatal, distinctly emanated from this cause. Everybody is familiar with the nuisance created by cesspools when vent is not given to their fetid contents; and does it not seem strange that individuals of moderate intelligence should, from want of a little attention, actually carry for days within them a nucleus of pollution, a cesspool in miniature? It is to inhabitants of populous towns, who lead a sedentary life, and are much confined within doors, that the above remarks especially apply; and without more than strenuously urging here the paramount importance of air, exercise, and diet, in obviating costiveness, I have briefly directed the notice of the Society to a medicinal agent not heretofore employed in this country, and which may be found serviceable in promoting the above purpose.

Medical Times, Aug. 11, 1849, *p.* 120.

164.—*On the Uses of Cod-Liver Oil.*—On looking over the various treatises which have been published on this subject, and on weighing the opinions and statements which have been expressed in them, it is indeed abundantly evident that cod-liver oil is an agent of no inconsiderable power. It is also evident that its action is chiefly in one direction, and is exerted almost entirely in modifying, restoring, and improving the several processes partly of primary, but chiefly of secondary assimilation, the derangement of which constitutes the

basis, as it were, and origin of certain symptoms which we are accustomed to treat, under various names, as special diseases. In dyspepsia, that is, in diseases resulting from some defect of primary assimilation, its effects have not been very accurately studied, chiefly perhaps from the difficulty of finding pure and uncomplicated cases of this affection. It appears to be most useful in the atonic varieties, and in those cases of inflammatory gastro-duodenal dyspepsia, in which, after reduction of the more prominent symptoms, the digestive powers of the stomach and duodenum recover themselves with difficulty. But in any cases in which the dyspeptic symptoms are dependent on more serious lesions, on ulceration of the stomach, or on incipient organic disease of the walls, &c., the cod liver oil appears to be unsuited, and even if retained on the stomach, to be inefficacious, or perhaps hurtful. It is chiefly in affections proceeding, as far as we at present know, from secondary mal-assimilation (to use the term of Dr. Prout), that the oil appears most useful. Such appears to be the case in chronic rheumatism, in rickets, in the various affections which are the local manifestations of the scrofulous diathesis, scrofulous caries, ophthalmia, tabes mesenterica, tubercular peritonitis, &c ; in phthisis pulmonalis, especially in the early stages; and in various chronic eruptions of the skin, as eczema, herpes, and some forms of impetigo. It appears to be more useful in these diseases of the skin, according as they can be traced back to some disorder of nutrition, and more especially if this borders on the condition which occurs in the scrofulous diathesis.

In addition, the oil has been used in a vast number of other affections, with an amount of benefit less marked and determinate. In true gout, psoriasis, porrigo, osteomalacia, in atonic amenorrhœa, in worms, and in the various sequelæ of smallpox, measles, and typhus, the opinions of its utility are very contradictory. It has appeared to be possessed of little power in Bright's disease, diabetes mellitus, and some other deep-seated and obscure affections. Cod-liver oil has also been found useful in cases in which tonics are indicated; as in convalescence after influenza, bronchitis, pneumonia, pleurisy, and other cases in which the disease having been partially or entirely removed, the constitution requires to have cautiously supplied to it both materials from which its impaired tissue may derive new force, and tone by which those materials may be effectively assimilated.

An opinion has been advanced, on very insufficient grounds however, that chronic rheumatism and tubercular consumption are in some degree antagonistic to each other. But independently of other arguments against such a view, we might be led to question its correctness by observing that the two diseases, or series of diseases, over which cod-liver oil appears to exert a considerable action, are chronic rheumatism and scrofulous affections. While in most other cases the benefit resulting from the use of the oil appears to be simply owing to its common tonic powers, in the above-named diseases there almost appears to be something specific about its action.

British and Foreign Medico-Chirurgical Review, Oct. 1849, *p.* 480.

165.—*On the Nutritive Properties of Fish Oil.*—By Robt. Druitt, Esq.—Of the virtues of cod-liver oil there can be no question;—and it seems capable of doing two things. In the first place it fattens, and adds to the bulk of the body: and, in the second place, it gives nutrition a better turn, as it were; it makes the fluids and solids healthier as well as bulkier, and enables them to throw off a variety of cachectic derangements. These useful qualities have been partially accounted for on the supposition that they are due to a minute quantity of some biliary principle contained in the oil. This supposition seemed to me extremely improbable, especially on considering the numerous adulterations to which the oil was liable; and accordingly I determined on making a few experiments on the subject, the results of which I beg to forward to you.

For this purpose I applied to my oilman for some specimens of the purest and sweetest lamp oil, and procured several varieties of whale and seal oil, decidedly fishy and rank in flavour, but not rancid or oxydized or putrescent. In fact, the flavour of the oil commonly called "southern oil," the produce of the black whale, which I chiefly employed, is not disagreeable to any one who is free from fancies on the subject; and if mixed with three or four parts of almond oil, is not a whit more offensive to the taste than the common oleum jecoris aselli.

Case 1 and 2. Two brothers. S., aged 3 and 5, flabby pasty children, each suffering from pustular eruption on the head and face. A wound made on the head of one of them a week since had degenerated into a flabby sore. No deficiency of food. Both took a tea-spoonful of seal oil three times a day in lemonade. Their mother reports that they were excessively fond of their medicine; they took it for a fortnight, when the skin of each was quite healthy, and complexion clear.

3. J. W., a pale unhealthy child, aged 2½ years; subject to pustular eruptions on the face. Cured by the same dose of southern oil, thrice daily for a week. Cured far more readily than on former occasions by calomel. Likes the oil extremely well.

4. J. L., a miserable child; glands in neck greatly enlarged; purulent discharge from ears; abdomen swelled and hard. This child got better under the use of seal oil, but did not take it regularly enough to make the case of any value.

5. J E., aged 2, subject to skin disease from birth; his mother has had syphilis; his complexion peculiarly pasty and sallow. Took southern oil in the above doses for a month. Greatly improved in flesh and complexion; but at the end of the course had an attack of eczema in the arms.

6. W., æt. 30; subject to sciatica. Took the southern oil; is certain that it has done him much good.

7. J. W., æt. 36. Was largely bled for acute rheumatism a twelvemonth since. Has never recovered flesh or strength, and is racked with pains in the neck and shoulders. Took cod-liver oil for a month with benefit last May; left it off during the summer; became thinner and weaker. Took southern oil in the dose of two

drachms thrice daily for three weeks; likes it much; feels stronger and looks as decidedly fatter and better in condition as he did from the cod-liver oil.

8. Mrs. P., suffered from puerperal mania whilst suckling, last autumn; has continued anæmic and despondent; has taken every form of mineral and vegetable tonic with temporary benefit. Took southern oil for three weeks; is unmistakeably plumper, clearer in complexion, and in better spirits.

9. J. M., a sallow child, æt. 4. took the southern oil for a week, for impetiginous eruptions on the face and legs. The improvement in flesh and clearness of complexion was extraordinary, and the eruption nearly disappeared.

These few cases do not prove much; but, so far as they go, are satisfactory. No one who had seen the children above mentioned before and after their course of oil, could doubt that a most beneficial change had been wrought by something. The great delight which the little wretches took in their dose is another point worth notice. I would therefore suggest, that it is well worth while to make a fair experiment on a large scale, to determine whether it is fish oil in general that does good, or only the oil of the cod's liver. If, as I believe, almost any kind of fish oil will answer the purpose, then many of the poor will be able to use the cheaper kinds, who could not afford the nicer but more costly cod-liver oil.

Medical Gazette, Nov. 9, 1849, *p.* 820.

166.—*On the value of Cod-liver Oil in the Treatment of Phthisis.*—[At the Brompton Hospital for consumption, cod-liver oil has been given very extensively, and in 542 cases its effects have been noted.]

Of these 542 cases, 293 were in the first stage of the disease, and 249 in the second and third, or those stages subsequent to softening. Of those in the first stage 190 were males, and 103 were females: 72 per cent. of the males, and 62 per cent. of the females, had their symptoms materially improved; in nearly 18 per cent. of the males, and in 28 per cent. of the females, the disease was arrested (the term *arrest* implies than all or nearly all of the symptoms of the disease had disappeared, the patients felt themselves well and able to pursue their ordinary occupations); in 10 per cent. of the males, and in nearly 10 per cent. of the females, the disease progressed unchecked, Of the 249 patients in the second stage of the disease, 139 were males, and 110 females; in 53 per cent. of the males, the symptoms were materialy improved, and in nearly 61 per cent. of the females. In a little more than 14 per cent. of the males, and in nearly 14 per cent. of the females, the disease was arrested. In a little more than 32 per cent. of the males, and in 25½ per cent. of the females the disease was not arrested. Viewing these results collectively, we find in about 63 per cent., the symptoms improved; in 18 per cent. the disease was arrested; and in 19 per cent., it went on unchecked. When it is recollected that, of the whole number treated at this Hospital, the disease was arrested in only 5 per cent.,

the value of this remedy, under which the disease appears to have been arrested in 18 per cent. of the cases, must be considered very great.

Different qualities of oil were tried, without exhibiting any marked difference in their remedial effects; but the offensiveness of some of the darker kinds rendered their general use impracticable. The oil now used is straw-coloured, transparent, and free from offensive smell. Patients in general take it without repugnance. The dose at first is 1 drachm three times a day for an adult; but it is gradually increased in some few cases to $1\frac{1}{2}$ oz. for a dose. It is usually administered in camphor-water, any aromatic water, bitter infusions, milk, or any other agreeable fluid. When there is great irritability of stomach, it has been given in mucilage of gum with a few drops of hydrocyanic acid. In cases where there existed great anæmia and debility, and in those where the effect of the oil seemed slight, preparations of quinine and iron, especially the iodide, have been conjoined with advantage. It has appeared advantageous to intermit its use for a few days when nausea and feverishness, from whatever cause produced, are present. In certain cases, the use of the oil has been continued during the existence of slight hæmoptysis, without producing any injurious results.

Other animal oils (not derived from the liver) and vegetable oils, were tried with a view of ascertaining how far their operation resembled that of cod-liver oil. The experiments hitherto made have not shown them to possess the same powers; but they have not been as yet sufficiently often repeated to warrant decided conclusions.

One of the most striking effects of the use of cod-liver oil is an increase in the patient's weight.

It must be observed, that an amelioration of the symptoms did not invariably follow an increase of weight, though the exceptions were rare. An aggravation of the symptoms and a diminution of weight were almost invariable coincidences. In a few cases the symptoms improved, though the weight remained stationary, or even became slightly diminished. In other cases, where the amelioration was still more considerable, and the progress of the disease appeared to have been stayed, relapse occurred, and was followed by a rapid progress to a fatal issue. That such cases do occur requires to be remembered, in order to restrain too sanguine expectations, and to prevent the remedy from falling into the discredit which disappointment after an unlimited confidence may induce. On the other hand, without entering into a description of the successive steps of amelioration experienced by patients, it will suffice to say, that many of the cases included in the 18 per cent., in whom the disease is marked arrested, felt themselves as well as they had been before the attack of the disease.

From these facts, no other conclusion can be drawn than that cod-liver oil possesses the property of controlling the symptoms of pulmonary consumption, if not of arresting the disease, to a greater extent than any agent hitherto tried.—*The First Medical Report of the Hospital for Consumption and Diseases of the Chest, by the Physicians of the Institution. London*, 1849.

Monthly Journal, Nov. 1849, *p.* 239.

167.—*How to Cover the Taste of Cod-Liver Oil.*—Now that the swallowing cod-liver oil bids fair to become the fashionable mania of the day, it may be as well to state the simple and effectual means communicated by M. Fredericq, of disguising its abominable taste. This merely consists in masticating a morsel of dried orange-peel just before and just after swallowing the dose.

British and Foreign Medico-Chirurgical Review, July 1849, *p.* 299.

168.—*How to Remove Nitrate of Silver Stains.*—Accident first led M. Martinenq to the observation, which he has since repeatedly confirmed, that the stains produced by nitrate of silver on linen, &c., may be readily removed by wetting the linen in a solution of *bichloride of mercury* (1 part to 31), rubbing it well, and then washing it in cold water.

British and Foreign Medico-Chirurgical Review, July 1849, *p.* 299.

169.—*On the Properties of Aconite.*—By M. Teissier.—M. Teissier has undertaken a prolonged series of researches in order to establish, if possible, the therapeutical value of this substance, and especially its *anodyne* and its *antiphlogistic* power. The existence of the former is incontestible, but it differs from opium in giving relief, not as that does to all descriptions of pain, but only to certain kinds of it. This speciality of the action of aconite is one of its characteristics, its anodyne property being only in fact a secondary one. Its principal, and in some measure specific, action is exerted upon the *skin*, eliminating through its vessels injurious principles, and restoring the natural functions when these have been disturbed. Thus aconite is a very appropiate remedy in diseases induced by chills, or when morbid principles are detained in the cutaneous tissues, as in the exanthemata; and M. Teissier has especially derived benefits from its anodyne properties in diseases of a rheumatic or catarrhal origin. The antiphlogistic power of the substance is quite subordinated to the influence exerted by it upon the skin, and is especially observed in individuals of a nervous or lymphatic temperament, and those predisposed to rheumatic and cutaneous affections; and in such it is beneficial in lumbago, angina and bronchitis, influenza, rheumatism, especially when recent and not very acute, &c. M. Teissier prefers the *tincture*, to obtain the anodyne effect of which he rapidly increases the dose from ten to twenty drops, to one or two drachms per diem.

British and Foreign Medico-Chirurgical Review, July 1849, *p.* 299.

170.—*On Pharmaceutical Preparations of Manganese.*—By M. Hannon.—*Oxide of Manganese.* This is a very good preparation, especially when obtained by the humid method; it should therefore be made only when it is wanted for use. The best mode of prescribing it, is to add to an ounce of simple syrup half a drachm or

a drachm of the hydrated oxide, with some oily emulsion, to prevent the contact of the air.

Carbonate of Manganese is best prepared by dissolving seventeen ounces of pure crystallized sulphate of manganese, and nineteen ounces of carbonate of soda, in a sufficient quantity of water. Double decomposition takes place; one ounce of syrup is added to every seventeen ounces of the liquid, and the precipitate is allowed to settle, in a well-stopped bottle. The supernatant fluid is then decanted off; the precipitate is washed with sugared water, and allowed to drain on a cloth saturated with simple syrup; it is then expressed, mixed with ten ounces of honey, and rapidly evaporated (the access of air being prevented) to a proper consistence for making pills. The sugar and honey oppose the transformation of carbonate of the protoxide of manganese (*carbonate manganeux*) into carbonate of the peroxide (*carbonate manganique*), which is but little soluble in the acids of the stomach. The dose is from four to ten pills, each four grains, every day in chlorotic cases, where iron has not succeeded. The hyperoxidation of the carbonate of manganese may be prevented by adding freshly prepared vegetable charcoal to the pills; it absorbs the carbonic acid which is disengaged by a partial decomposition, and enables the pharmaceutist to dispense with the use of mucilage, which only increases the hardness of the mass.

Neutral Malate of Manganese. This is procured by treating carbonate of manganese with malic acid. It is an eligible preparation, as the base of the salt is in the form of protoxide, and the acid is easily digested. The dose is from two to four grains, in pills.

The preparations of manganese have this immense advantage over those of iron, that they can be combined with vegetable tonics and astringents, namely, tannin, and the substances which contain it, as gall-nuts, rhatany, catechu, dragon's blood, kino, monesia, canella, and cinchona. These can all be combined with malate of manganese. *Syrup of malate of manganese* consists of, simple syrup, ℥xvi; malate of manganese, ℥i; essence of lemon, ʒij: an ounce of syrup contains 29 grains of malate of manganese. *Pills of malate of manganese.* Malate of manganese, gr. xv; powder of cinchona, gr. xv; honey, a sufficient quantity to make twenty pills. *Lozenges of malate of manganese.* Malate of manganese, ℥i; sugar, ℥xi; mucilage of tragacanth, a sufficient quantity. To be formed into lozenges, each twelve grains in weight; each of which contains a grain of the salt.

Tartrate of manganese is prepared in the same way as the malate, tartaric acid being used. It may be substituted for the malate in all the above mentioned formulæ; and is used to prepare the following highly tonic syrup. Syrup of tolu, ℥xvii; extract of rhatany, ʒiiss; tartrate of manganese, ʒiiss. Dose, from four to five spoonfuls daily.

Phosphate of manganese is best prepared by dropping a solution of phosphate of soda into a solution of sulphate of manganese. The precipitate is collected after filtration, dried, and preserved in well-stopped bottles. This preparation may be employed, like the

phosphate of iron, in cancerous affections. *Pills of phosphate of manganese.* Phosphate of manganese, ʒiss; powder of cinchona, ʒss; syrup of catechu, a sufficient quantity. To be divided into four-grain pills. *Syrup of phosphate of manganese.* Phosphate of manganese, ʒss; syrup of tolu, ℥iii, ʒiii; syrup of cinchona, ℥v; essence of lemon, ʒiss; powder of tragacanth, gr. x. This preparation must be made quickly, and preserved in a well-stopped bottle. *Lozenges of phosphate of manganese.* Phosphate of manganese, ℥i; sugar, ℥xii. Mix and divide into twelve-grain lozenges, each containing one grain of the phosphate.

Iodide of Manganese is prepared by digesting recently precipitated carbonate of manganese with fresh hydriodic acid; then filtering, and evaporating, the access of air being prevented. It may more conveniently be prepared extemporaneously, by mixing together an ounce of iodide of potassium, and the same quantity of sulphate of manganese, perfectly dried, and in the state of powder. It is then made into a pill-mass with honey, and divided into pills, each containing four grains of the iodide; which should be kept in a well-stopped bottle. The dose is at first one pill daily, gradually increased every three days, to six pills; the medicine is then omitted for eight days, after which it is resumed again. *Syrup of iodide of manganese* is prepared by adding concentrated hydriodic acid to a drachm of perfectly pure hydrated carbonate of manganese, until it be entirely dissolved; then mixing with the solution 17 oz. of a syrup of guaiacum and sarsaparilla. Dose, from two to six spoonfuls daily.

In cases where iron has not succeeded, it is desirable not to make a sudden transition to manganese, but to combine the two remedies as in the following formula. Pure crystallized sulphate of iron, ℥xiii.; pure sulphate of manganese, ℥iiiss.; pure carbonate of soda, ℥xviiss.; honey, ℥x.; syrup as much as may be sufficient to make a mass, to be divided into four-grain pills. Dose from two to ten pills daily. The insoluble preparations of manganese should be first used, as the carbonate, phosphate, and oxyde; then the more soluble preparations, the tartrate, malate, &c., may be employed. The use of this medicine should not be persevered in so long as that of iron, as its preparations are more rapidly assimilated. Manganese is not, like iron, found in the excrements of persons who take it—at least it is in very small quantity.

In the depraved state of the blood which succeeds intermittent fevers, manganese is useful; it is the most certain remedy for preventing a return of the attacks. Leucophlegmasia and engorged spleen, of long duration, are rapidly reduced by the use of iodide of manganese with syrup of cinchona. The preparations of manganese should also be used in urethro vaginal catarrh in chlorotic patients, and in chronic blennorrhœa, especially in individuals weakened and rendered anæmic by excess. The salts of manganese with which we are acquainted, are powerfully astringent, and may be used as external applications, in all cases where other astringents are not indicated. In this respect they possess no other peculiarity.

London Journal of Medicine, Sep. 1849, p. 881.

171.—*On Cantharidal Collodion for Blisters.*—[M. Hisch, of St. Petersburgh, recommends the following preparation as a vesicatory, when it is needed to apply one where it will not be disturbed by the movements of the patient.]

The cantharidal collodion is thus prepared:—Exhaust, by the displacement process, one pound of coarsely powdered cantharides by one pound of sulphuric ether and three ounces of acetic ether: in this way is obtained a saturated solution of cantharides, along with the green animal fatty matter. Dissolve twenty-five grains of cotton-powder in two ounces of this liquid, and preserve it for use, in well stoppered bottles. When a blister is required, it will be sufficient to smear with this fluid the surface to be vesicated.

London Journal, Oct. 1849, *p.* 962.

172.—*Papaverine, a New Alkaloid obtained from Opium.*—Dr. G. Merck has obtained from the residuum left after the preparation of morphia, a new alkaloid which he calls papaverine. It presents prismatic crystals of a very regular form, and quite white; very soluble in warm ether or alcohol, but hardly soluble in these liquids when cold, and quite insoluble in water. Papaverine reddens litmus paper, turns dahlia paper green, and forms well determined salts with acids; the hydrochlorate being particularly remarkable for the beauty of its crystals. When papaverine is rubbed up with concentrated sulphuric acid, it assumes a very characteristic tint. When a large excess of hydrochloric acid is poured on a solution of papaverine, a very heavy oil is thrown down, which forms in a few hours a crystalline mass; this dissolves in boiling water, in which, on cooling, it forms prismatic deposits which have no acid re-action. When these prisms are again treated by an excess of hydrochloric acid, they produce the same oil obtained before. This latter may, however, be thrown down by using either sulphuric or nitric acid in the above-mentioned manner. The chloroplatinate of papaverine is yellow, pulverulent, and insoluble. The formula of papaverine, calculated according to the composition of the hydrochlorate and chloroplatinate is C_{40} H_{21} N O_8 — *Gazette des Hôpitaux.*

Lancet, Aug. 4, 1849, *p.* 125.

173.—*Use of the Leech as a Weather-Prophet.*—By W. H. Attree, Esq., late House-Surgeon to the Middlesex Hospital.—[The poet Cowper has mentioned the leech as a weather-prophet, asserting that "no change of the weather surprises him, and in point of the earliest and most accurate intelligence, he is worth all the barometers in the world." Mr. Attree has verified the truth of this statement, and gives the following observations which he has made upon the subject:]

1. If the weather proves serene and beautiful, the leech lies motionless at the bottom of the glass, rolled together in a spiral form

2. If it rains either before or after noon, it is found crept up to the top of its lodging, and there remains until the weather is settled.

3. If we are to have wind, the poor prisoner gallops through its limpid habitation with amazing swiftness, and seldom rests till it begins to blow hard.

4. If a remarkable storm of thunder and rain is to succeed, for some days before it lodges almost continually without water, and discovers uncommon uneasiness in violent throes, and convulsive-like motions.

5. In the frost, as in the clear summer weather, it lies constantly at the bottom; and in snow, as in rainy weather, it pitches its dwelling upon the mouth of the phial.

6. Perhaps it may not be amiss to state, lest any of the curious should try the experiment, that the leech was kept in a common two-ounce phial about three-fourths filled with water, and covered with a bit of linen rag. In the summer the water is changed once a week, and in the winter once a fortnight. This is a weather-glass which may be purchased at a trifling expense.

Medical Gazette, July 6, 1849, *p.* 41.

174.—*On Animal Charcoal as an Antidote to Poisons.*—By Dr. B. H. Rand, Philadelphia.—In the "Transactions" of the Medical Society of London, new series, Vol. i., is a paper, by Dr. A. B. Garrod, detailing some experiments, in which he employed purified animal charcoal as an antidote. This is prepared from ivory black, by digesting it in dilute chlorohydric acid to remove the earthy matters, afterwards washing and heating it to redness in a covered crucible. The subjects of his experiments were dogs, rabbits, and guinea-pigs. He administered large doses of opium, belladonna, aconite, nux vomica, delphinium, stavesacre, white hellebore, and their alkaloids, as well as digitalis, hemlock, tobacco, elaterium, ipecacuanha, hydrocyanic acid, cantharides, and arsenious acid, without injurious consequences, when sufficient animal charcoal was given simultaneously with them, or, in some cases, before the peculiar effects of the drug were developed. It also prevented, but less completely, the action of the bichloride of mercury, and of the salts of copper and lead

Dr. Garrod concludes from his experiments:—

1st. That animal charcoal has the power of combining in the stomach with the poisonous principles of animal and vegetable substances, and that the compounds thus produced are innoxious; therefore, when given before these poisons have become absorbed, it will act as an antidote.

2d. That animal charcoal will absorb some mineral substances, and render them inert; but so large a quantity of the charcoal is

required, that it is not so well adapted for many poisons of this class as their own special antidotes; the effects of arsenic, however, appear to be better combated by this than by any other article.

3d. That a certain amount of animal charcoal is required, about half an ounce to each grain of morphia, strychnia, or any other alkaloid; but, of course, much less for the substances from which they are obtained, as opium, nux vomica, &c., a scruple of nux vomica not requiring more than half an ounce of charcoal.

4th. That the antidote itself exerts no injurious action on the body.

The method of preparing pure animal charcoal from ivory black, before mentioned, is tedious and wasteful, only ten per cent. being obtained, while the amount of acid used is considerable. A very good and pure charcoal is obtained by calcining leather scraps or blood with pearl-ash, washing and re-heating in a close crucible—it was this kind of charcoal which was principally used in the experiments to be detailed. Much of the discrepancy of experimenters with this substance is undoubtedly due to their use of an impure specimen of it.

With a view of ascertaining more accurately the value of the proposed antidote, the writer performed a number of experiments, from which the following are selected as having the most direct bearing upon the subject:—

1. One grain of pure morphia was swallowed with about an ounce of pure animal charcoal, in warm water; no narcotic symptoms supervened, but there was some gastric irritation, which subsided in the course of the day.

2. One grain of sulphate of morphia was digested with pure animal charcoal until all bitterness was removed; the liquid, filtered off and swallowed, produced no effect.

3. Ten grains of extract of belladonna were swallowed with two drachms of the charcoal; there followed vertigo, dilated pupils, dimness of vision, exceeding dryness of the throat, and desire to sleep, all of which symptoms were relieved by the spontaneous vomiting of a very acid matter, and the use of stimuli. The pupil remained dilated through part of the next day.

4. The last experiment was repeated, an antacid having been premised, and the proportion of charcoal being doubled. Some dryness of the throat followed, but with no other symptom of the influence of the drug.

5. Fifteen grains of powdered digitalis were taken, with three drachms of the animal charcoal, without the slightest disturbance of the functions.

6. Twelve drops of the officinal hydrocyanic acid were swallowed, with two drachms of the pure charcoal, without a sedative result.

7. One grain of strychnia, dissolved by the aid of a drop of chlorohydric acid, was digested with animal charcoal until all bitterness was removed. The solution, filtered and swallowed, produced

no effect; a similar solution, evaporated and tested with nitric acid, gave no red tinge.

8. One grain of strychnia was swallowed, with an ounce of pure animal charcoal; no effects due to the strychnia could be perceived.

9. The purgative extracts were next tried, but produced no effect when sufficient animal charcoal was taken.

10. Camphor and musk were removed by animal charcoal from their tinctures, so far that they did not precipitate on the addition of water.

11. Phosphorus was removed from its ethereal solution by the charcoal.

12. Iodine was so far removed from its tincture and compound solution as not to strike a blue colour with starch; and the iodine could not be liberated from the animal charcoal at a red heat.

13. Arsenious acid and a solution of arsenite of potassa were apparently unaffected, either in the hot or cold solution, by animal charcoal. This result, although it agrees with those of MM. Wapen and Graham, does not with the observations of Dr. Garrod, who states that animal charcoal "has greater power of removing arsenic from its solution than the hydrated sesqui-oxide of iron."

14. A solution of the bichloride of mercury, being treated with animal charcoal, gave, on filtration, no precipitate with ammonia.

In conclusion, we are perhaps justified in drawing, from the present state of our knowledge on this subject, the following conclusions:—

1st. That animal charcoal has the power of withdrawing, when used at a proper temperature and in sufficient quantity, most, if not all, known vegetable and animal poisonous principles, and certain mineral poisons from their solutions.

2d. That, given at the same time with, or shortly after these poisons have been swallowed, it prevents their deleterious action.

3d. That given in cases of poisoning, it can exert no injurious influence, but, on the other hand, promotes vomiting, entangles the poison, and protects the coats of the stomach against it.

4th. That although it cannot be substituted for the usual antidotes in poisoning by mineral substances, yet it may be usefully employed in conjunction with them, or in their absence.—*Medical Examiner*, Sept. 1848.

Monthly Retrospect, June 1849, *p.* 133.

175.—*Calcined Magnesia an Antidote for Arsenic.* — By Dr E. Bissel, Norwalk, U. S.—[Dr. Bissel was sent for to a young man, 27 years of age, who had taken not far from a scruple of arsenic. When he arrived, the patient had taken emetics of sulphate of zinc and of infusion of tobacco, and had vomited freely. At this time, Dr. B. says:]

His pulse was one hundred and thirty per minute, small and wiry. He complained of great constriction and dryness of the fauces, but

chiefly of a most agonizing pain and burning in the stomach; it seeming, as he expressed it, "as if it were filled with burning coals." As nearly three hours had now elapsed since the poison had entered the stomach, I considered that any further effort to evacuate it would be futile, and, that if life was saved at all, it must be by the antidotal power of some medicinal agent. Having had my attention directed to the experiments of Professor Peter, of Transylvania University, and the case of Lepage published in January and April numbers of this Journal, in 1847, I determined to give the calcined magnesia a fair trial, and accordingly put it up in drachm doses, to be given every hour mixed in milk and water. During the hour which I remained with him, his symptoms were becoming more unfavourable. The pulse was one hundred and fifty per minute; the constriction and dryness of the fauces extreme; the whole surface bedewed with perspiration; the pain and burning sensation in the stomach seemed augmented to the highest possible degree, whilst the right hand was entirely paralysed; in short everything betokened a speedy dissolution. I left him at 10 o'clock, in charge of the mistress of the family, whom I knew to be a faithful and intelligent nurse. On visiting him the next morning, instead of finding him dead as I much feared, I was most happily surprised to find him very quietly dosing in an easy chair. I learned from the lady, who had been unremitting in her care of him during the whole night, that in a very few minutes (not more than five or ten) after taking the first dose of magnesia, he said he felt much relieved, and before the time for the second dose, he had fallen into a doze. She stated that each successive dose had produced the most surprising and marked mitigation of every symptom, and that long before morning he was entirely freed from suffering, and had, on the whole, passed a quiet and comfortable night. The bowels had moved freely and easily twice during the time. He complained of nothing save a general weakness, and a sort of faintness at the pit of the stomach. The right hand had recovered its power, and the pulse had fallen to eighty-five per minute. Directed to continue the magnesia through the day, once in four hours, and to give light nourishment. I have since learned, that in a very few days he resumed his labour upon the farm, and felt no inconvenience from what he had taken, except a muscular weakness of the lower extremities, which was not very great.

Monthly Retrospect, June, 1849, *p.* 135.

176.—*On the Use of Dutch Liquid.*—By Dr. J. Snow.—Dutch liquid is much slower in its action than chloroform; and whilst the chief endeavour in giving chloroform is to prevent the air from getting too strongly charged with the vapour, in giving Dutch liquid the endeavour is to get the air to take up sufficient of it. In one case, indeed, that of an infant in King's College Hospital, on which Mr. Fergusson operated for nævus, it failed to induce insensibility with the inhaler I was using (one contrived for chloroform), although continued for three or four minutes, and rather than cause further delay chloroform was used.

For reasons similar to those which render Dutch liquid slower in its action, when its effects are once produced they are more persistent than those of chloroform. Medicines so volatile as these escape from the system almost exclusively by the lungs; and as the quantity of Dutch liquid in the blood during insensibility is greater than that of chloroform, it would be longer in escaping, even if it could be exhaled at the same rate; but, being less volatile, it cannot. There is a continued tendency to equilibrium between the elastic force of the vapour in the blood and that in the air contained in the pulmonary cells: and if the blood contain, for instance, one-thirtieth part as much of a volatile liquid as it could dissolve, each cubic inch of air which reaches the cells of the lungs is capable of taking up one-thirtieth part as much as would saturate it at 100°; but this quantity is twice as great in the case of chloroform as in that of Dutch liquid. The longer duration of the effects of the latter substance as compared with the former has been very marked in a number of experiments on animals, as well as in practice.

Although, as above stated, a greater quantity of Dutch liquid than of chloroform is required to induce insensibility in the first instance, yet in cases requiring the continued inhalation of the vapour there is but little difference in the amount used; since, from the more persistent effect of Dutch liquid, it does not require to be repeated so often.

The result of my observations and investigations is, that I cannot unite with Mr. Nunneley in his general praises of Dutch liquid. The only advantages which it possesses over chloroform, in any case, are such as are connected with its slower action and more persistent effects,—properties that Mr. Nunneley failed to recognize. In all other respects its effects appear to be the same as those of chloroform. It is undoubtedly a very safe anæsthetic; but I doubt very much whether practitioners would be content to wait for its slower action, after they have been accustomed to use chloroform, even if it could be obtained at the same cost, of which there is no prospect. In whatever way Dutch liquid might be used, it would not suddenly occasion a fatal accident without giving due warning; in this respect it resembles ether. Advantage might be taken of its more persistent effect in some operations in the face, in which it is difficult to administer a vapour after the surgeon commences; and also in cases where the operator is without an assistant, and has to make his patient insensible first, and then to perform his operation. In obstetric practice it would perhaps be more convenient than chloroform, when only one medical man is present, as he might intrust the inhaler to the nurse, and look up two or three times in a minute to give directions; but when there is a practitioner entirely to superintend the inhalation, chloroform has the advantage, as it can be given to the requisite extent just as each pain commences, and the patient can be allowed to recover from its effects, more or less, between every pain.

Medical Gazette, Aug. 17, 1849, *p.* 274.

177.—ON A FORM OF FATTY DEGENERATION OF THE HEART.

By Dr. E. L. Ormerod.

[At p. 114, of our Sixteenth Vol., will be found an interesting article by Mr. Paget, on fatty degeneration of the heart. In the following paper, Dr. Ormerod, who was Mr. Paget's fellow-labourer in this inquiry, enters more into detail concerning one form of the disease. He says:]

Fatty degeneration may occur under two conditions—viz. as a primary or a secondary affection. Primarily, it may occur in a part which is no longer wanted, as in the muscles of aged or crippled limbs, and in the structure of crippled organs. Such a change we see best in the bodies of some old people. The parts which are no longer wanted are not simply atrophied, but converted gradually into fat, and the blood apparently is so overloaded with this ingredient that it fails to remove the fat from those parts which are in earlier years naturally padded or distended with this substance. So the body retains, in many respects, the form and plumpness of youth, but the texture is flabby, and the colour pale, for the want of blood and muscle to give it the hue and firmness of youthful life.

The other condition frequently connected with this peculiar form of atrophy, which may then be called secondary, is the previous occurrence of chronic inflammation in this part. Here, as in the former case, we must look to the general habit of the individual for some share of the explanation; but the fact of the one being occasionally ingrafted on the effects, or accompanying the progress of the other, rests on the best evidence. This is most commonly seen, as might be expected, in internal organs. Probably the same cause which has induced inflammation, has itself, in a very large majority of cases, given the tendency to fatty degeneration of the particular organs.

Of the nature of this fatty degeneration, under whatever circumstances, there can be no doubt: it is essentially an atrophy, whereby the most highly organized elements of the body are replaced by one of the simplest. It is not that the substances are changed into fat, that is plainly impossible, but that the nutritive processes of muscle, or gland, or bone, no longer restore particle for particle whatever is lost by the daily use of the parts, but replace it with oil. Nutrition goes on in some sort, but the additions are of matters wholly unsuited to the office of those that they have replaced: they can neither move nor secrete, nor even mechanically support the weight of the body.

There are three forms in which fatty disease of the heart may occur:—

1st. In the first form the fat is accumulated in those parts where it is naturally deposited in the greatest abundance, as at the base of the heart. This is met with in those persons who have a general tendency to accumulate fat; and to this form, probably, most of what are called fat hearts belong. It is certainly a condition of

disease, but probably not an important one, and needs no additional illustration here.

2d. In the next form the fat collects chiefly about the apex of the heart (Laennec, Auscult. Médiate, tome iiii. p. 225). The deposit begins from without, and is not attended with so great an enlargement of the heart as in the form first described; for the fat is deposited at the expense of the muscular tissue—in its place, not merely laid upon it. Moreover, the degeneration is partial, and in patches, not general. This tendency to limit itself to particular spots is seen most characteristically in those cases where a single carnea columna is converted into adipose tissue, but still preserves the form of the muscular band whose place it has usurped. This form occurs in persons who have, as is popularly said, an unhealthy disposition to grow fat; in those where the fat has generally throughout the body, as in the heart, a tendency to accumulate in the place of, not in addition to, the proper structures. The disease is, in some sense, a degeneration in this form, but not perhaps so strictly as in the form remaining to be described, the proper subject of these remarks. To this second form belong nearly all the cases, as far as I am able to judge, described as examples of fatty disease of the heart. This fact in itself bespeaks the much greater importance of the second than of the first form; for there the change in the heart is commonly described as a morbid appearance among many others; here it is the change on which the interest of the case is made to turn.

Dr. Cheyne, and other Dublin practitioners, among whom the disease has excited a good deal of interest, have recorded cases of this description. But instead of repeating one of their well-known cases, the following, for which I am indebted to my friend Dr. Kirkes, will serve as an excellent illustration:—

Sudden death—fatty degeneration of the heart.—A German clock-maker, a powerful, well-built man, applied for admission at St. Bartholomew's Hospital, in August, 1847, and he looked so pale and ill that the only empty bed in the house was given him, though a fever patient had to be refused admission.

His friends said that he had always had remarkably good health, and that he was a man of great strength and powers of endurance. But within the last year he had been subject to occasional fits, during which he lost consciousness, fell, and struggled for a short while. He had one such fit ten days ago. His face was now pale; his expression languid; his gait tottering; pulse thrilling, occasionally irregular. He said he had no uneasiness in the chest, only he had wandering pains, with a sense of fainting and debility.

He was put to bed, and lay quiet and unnoticed that evening and night. Early on the following morning the nurse found him quite dead in bed.

On examination of the body thirty-six hours after death, the limbs were still rather rigid. A large quantity of fluid blood escaped from the divided cerebral vessels; the brain itself was healthy.

Nothing particular was observed in the lungs, or in the abdominal viscera.

The heart was rather larger than natural: it was marked with two "white patches" externally; it felt like a loose bag, its walls being quite flaccid, and it was already partly decomposed, its walls interiorly being stained by the dark grumous blood which it contained. The right side of the heart was of the natural size, though the auricle was just then distended with blood. The left cavities were smaller than natural. On close examination, the heart appeared as if entirely overlaid with a layer of dirty yellowish fat, separated by a distinct boundary from the muscular substance of the organ; but the muscular tissue itself was pale, like dead leaves, contrasting forcibly with the bright red colour of his abdominal muscles.

The rapid decomposition which a portion of the heart set apart for the purpose underwent, vitiates any conclusions from the microscopic examination. A piece of abdominal muscle, from the same subject, had, in the same time, undergone no change.

For the present, let this brief summary and this single example suffice for this very common yet very terrible form of the disease.

The third form of disease was first, as far as I am aware, described by Rokitansky, who speaks of it as affecting the heart by preference; then, next in order of frequency, the voluntary muscles, and sometimes, though very rarely, the involuntary muscles, —as, for instance, the coats of the gall-bladder. The subject has been also investigated by Mr. Paget with his usual ability. To his description of the morbid appearances I have nothing to add, and have only to offer my humble confirmation of the correctness of his remark that the fatty matter is partly within, and not wholly external to, the sarcolemma, as described by Rokitansky. But I cannot omit to acknowledge the extreme pleasure and advantage I have derived from the assistance of Mr. Paget, as well on the occasion of working out this part of the subject, as always on other occasions, whenever the direction of his pathological researches has happily coincided with mine.

To the unassisted eye, the muscular substance of a healthy heart presents characters distinguishing it from ordinary muscular tissue; for it is more compact and homogeneous, and not loosely divided into bundles of fibres, as is ordinary muscle. Under the microscope it also presents some striking differences, the transverse striæ being less distinctly marked, and the fibres having a singular granular appearance. It is very important to notice this normal difference at the outset; for the first step towards fatty degeneration consists in the loss of continuity of the transverse striæ, and in the increase of this granular marking of the fibres, which would seem to be in some degree their normal appearance.

This is the first step, and, as wholly undiscoverable by the naked eye, may often pass unnoticed, unless something in the symptoms, or some change in the general condition of the heart, call particular

attention to that organ. Such conditions may be, a small, pale, flabby state of the heart, not inaptly compared to the colour of withered leaves, and to the feel of a moist glove. But such are not commonly the signs which call attention to the existence of this structural change: they are ordinarily much more obvious.

On opening a heart thus affected, the interior of the ventricles appears to be mottled over with buff-coloured spots of a singular zigzag form. The same may be noticed beneath the pericardium also; and in extreme cases the same appearance is found, on section, to pervade the whole thickness of the walls of the ventricles and of the carneæ columnæ. Of these latter the *musculi papillares* seem most liable to be affected. Not to say that this form of disease never occurs in the walls of the auricles,—at least, I have never seen it there.

Microscopic examination reveals the nature of these spots; they are not deposits, but distinctly degenerated muscular fibres; the outline, not merely of the masses, but of each single fibril, is accurately preserved. Instead, however, of transverse striæ and nuclei—the evidences of active vitality—there is little to be seen but a congeries of oil globules. The whole history of the degeneration may be traced in one of these little spots. First, from the immediate neighbourhood of the spot we may obtain healthy muscular fibre: then the transverse striæ becomes less distinct; they are rows of dots rather than continuous lines; then the intervals between the dots become wider, and the dots themselves run into longitudinal rather than transverse lines; and then all regularity is lost, and the dots appear to stud the surface all over, like the points on a bit of fish-skin. Probably long before this time the fibre has lost all its properties as a muscle: but there are further changes to observe; for now, mixed with these minute dots, are to be seen small oil-globules, which increase and coalesce till the fibril presents little else but a congeries of oil-drops contained within the sarcolemma.

This is not the only change which the fibres undergo; for, with whatever care they are disintegrated, they are found to be short, and as if unusually brittle—a general condition which may, perhaps, be of more serious importance than the actual fatty degeneration of the organ.

Such are the most common features of the disease, and sufficiently obvious, when noticed, to prevent their being readily overlooked afterwards. But we must not rely too exclusively upon them; for, as already observed, in the absence of these little spots marking the extreme degree of fatty degeneration in single points, the disease may have pervaded the whole substance of the heart; and the recognition of such a change will be the more difficult in exact proportion to its extent, and therefore its importance, from the want of healthy tissue wherewith to contrast the diseased fibres. And there is no solution to the difficulty except in the use of the microscope, whose information, should anything casually induce us to solicit it, on this subject at least is infallible.

It is perhaps almost needless to provide against such a misinterpretation as that this should be presumed to be an ordinary change

of the heart. The following, however, has been chosen, in this view as an illustration of what may be called fibrous degeneration of the heart, where, mixed with the atrophied muscular tissue, are the products of previous inflammation.

Angina pectoris—Sudden death—Aneurism of the left ventricle—fibrous degeneration of the heart.—A clergyman, aged 45, of sedentary studious habits, inclined to grow fat, though abstemious to a degree, for the last two years of his life was subject to paroxysms of pain in the region of the heart. The first paroxysm occurred on occasion of some mental anxiety; then he used to suffer them about once a month, and latterly almost daily.

There was no valvular murmur, only the heart's sounds were loud and ringing. He found more relief from constant mental employment, and from the practice of abstinence to a degree which most would call starvation, than from anything else. One morning he was found dead by the side of his bed.

On examination of the body after death, with Dr. Jeaffreson and Mr. R. Eyles, besides a few slight morbid changes irrelevant to the present subject,—

The heart was found large, weighing ℥ xij.; there was a slight degree of general vascular injection of the pericardium, which contained ʒ ij. of clear fluid. The right side was healthy. The left valves were efficient, but thickened. The walls of the left auricle were thickened; the left ventricle was rounded at the apex, which projected far beyond the right. In a space of about an inch in diameter, on the front of this ventricle, the walls were very thin, and could easily be made to bulge. The substance of the heart here was injected, but tough, and chiefly made up of cellular tissue. The carneæ columnæ hereabouts, and those leading to the right side of Lieutaud's valve, were pale and white, being composed in chief part of fibrous tissue, the fibres being long, straight, and parallel, mixed with only a few proper muscular fibrils. Elsewhere the fibres of the heart were long, straight, well coloured, and the transverse striæ well marked. There was no fatty degeneration anywhere.

The aorta was thick and uneven, with a few calcareous plates, and with the lining membrane removed in a few places.

In many respects, then, the anatomical history of fatty degeneration of the heart appears to be complete. We can trace it from its earliest beginnings to the entire disintegration of the muscular structure. It is a specific change with as distinctly marked characters as have any of those diseases of which we have cognizance by the eye or ear—tubercle or pneumonia, for instance. There are some points, however, which observation has yet to supply; for there are ulterior changes in the muscular structure of the heart—perforations, rupture, and apparent loss of substance,—which experience has not yet shown to be strictly the results of fatty degeneration, however probable the general circumstances may have made it that such will be found to be the case on more extended inquiry.

Medical Gazette, Nov. 2, 1849, *p.* 739.

178.—*On the Treatment of Psoriasis and Pityriasis.*—By Dr. J. M. Neligan, Physician to Jervis-Street Hospital, Dublin.—[While allowing the remedial efficacy of arsenic in these diseases, Dr. Neligan maintains that its beneficial action is more speedy and decided where it is employed together with iodine or iodide of potassium; and that the latter medicine alone is of great value in those cases in which arsenic disagrees. He relates the case of a gentleman affected for many years with inveterate psoriasis, to whom he gave this medicine with great advantage. He says:]

The plan of treatment which I prescribed for this gentleman consisted in the administration of five grains of iodide of potassium, dissolved in two ounces of a decoction of the fresh inner bark of the elm, taken every night at bed-time, and the employment of a fresh-water bath at the temperature of 92° F., at first three times, and afterwards twice weekly. In his case it is probable that the arsenical preparations might have acted equally beneficially, but he had an insuperable objection to take arsenic in any form; and from my previous experience of the good effects of the iodide of potassium in many cases where arsenic failed to cure the disease, I confidently adopted its use, and the result has proved that I did not overrate it as a remedy for inveterate psoriasis.

Donovan's solution of the hydriodate of arsenic and mercury has been extensively employed in the treatment of this class of cutaneous eruptions, and the pages of the former series of this Journal bear ample testimony to its good effects. I must however, confess that I am not partial to the employment of any mercurial preparation in their treatment, when they affect the body generally; and the only local form of scaly diseases, in which I have found a mercurial of use is in pityriasis of the scalp, as I mentioned in a former essay, and in it I still find the yellow iodide of mercury productive of the best results.

The iodide of arsenic has also been found a remedy of decided efficacy in the treatment of these affections; it was first generally employed for them by the late Dr. Anthony Todd Thomson of London, and the testimony of those who have used it is confirmatory of his observations. My own experience leads me to place more reliance on it than any other preparation of arsenic; but, although I have found it, when administered alone, capable of curing many cases of psoriasis and lepra, I consider that its beneficial action is much augmented by combining it with the iodide of potassium, and with iodine in the form I shall hereafter describe. The following case is selected from many, as a proof of the effects of this remedy in producing a permanent cure. It was reported by Dr. T. W. Christie, now of Sidmouth.

Daniel Callaghan, nineteen years of age, a baker by trade, of middle stature and sanguine temperament, was admitted into Jervis-street Hospital, February 7, 1843, affected with psoriasis inveterata. The greater part of his body, and particularly his legs and arms, is covered with an eruption consisting, in some places,

of small circular scales, and in others, of large irregular patches coated with white scales, and having an elevated border. There is an abundant desquamation of the cuticle, especially from the lower extremities. The scalp is also affected, but the spots there seem to be more circumscribed. The tongue is rather clean; the bowels constipated; the appetite good, but he states that his strength is latterly much diminished.

From his own account, he was always healthy previously to his becoming a baker (about five years since); eighteen months subsequently to which period, having lived intemperately while exposed to the great alternations of temperature inseparable from his trade, his present disease made its appearance. It came on in the form of minute, red, elevated spots, about the size of a small pea, extremely itchy, and rendered more so by heat; they did not exude any moisture except when severely scratched; scales soon formed on them, which continually fell off, but to be renewed by others; and, rapidly spreading, they soon covered the whole of his body, except the face and hands, which have but partially suffered. He has been treated by various medical men during the last three years, but he has never received more than partial benefit from the many remedies tried on him. On admission he was ordered a warm bath and purgative medicine, and the next day a twelfth of a grain of the iodide of arsenic, made into a pill with liquorice powder, was prescribed for him, to be taken three times daily. This treatment was continued, the dose of the iodide of arsenic having been gradually increased to a sixth of a grain three times daily, until the 3rd of May, when he was discharged from the hospital without a trace of the disease remaining. The medicine had occasionally to be omitted for a few days, when the system gave evidence of being saturated with it, and the symptoms then produced were readily removed by an active saline purgative, and the omission of the iodide, generally for two or three days.

This patient, during the entire time that he was in hospital, was kept strictly on milk diet.

This case is a good example of those in which the iodide of arsenic alone effects a rapid and permanent cure; but unfortunately in most instances, it does not produce such decided beneficial results; and when, after a fair trial of it, I find that the disease is not disappearing, I generally combine it with iodine and the iodide of potassium, or, omitting it altogether, give the latter medicine in the decoction of elm bark; and in very obstinate cases I administer them alternately, usually every second fortnight.

[After again insisting upon the importance of strict adherence to a milk diet, and mentioning the value of tepid baths administered twice or thrice a week, as adjuncts in the treatment, Dr. Neligan says:]

I have already stated that I have derived excellent effects in the treatment of squamous eruptions from a combination of the iodide of potassium with arsenic and with iodine. Commencing the

treatment, as I usually do, with the iodide of arsenic, where nothing forbids its use, after it has been continued for a month or six weeks, I order a mixture containing two drachms of the liquor arsenicalis, two drachms of simple syrup, five ounces and a half of distilled water, and half a drachm of iodide of potassium. Of this mixture, I prescribe, at first, a dessert-spoonful three times daily, in a glass of *eau sucrée*, directing it to be taken after meals. This quantity contains five drops of the arsenical solution, and a grain and a quarter of iodide of potassium. To it I sometimes, in very inveterate cases, or after it has been continued, in gradually increased doses, for some weeks, without decided effect being produced, add six grains of iodine,—a fourth of a grain to each dose. The mixture then, chemically speaking, is a solution of the iodurctted iodide of potassium and arsenic. When iodine is added in this proportion to the mixture, I use syrup undiluted as the menstruum, and then direct the dose to be taken in plain water. If a flavoured syrup be employed, the mixture is far from being disagreeable. After numerous trials made for me by Mr. Oldham, of Grafton street, I find that syrup of capillaire most completely conceals the odour and taste of the iodine.

The only other preparations of arsenic that I have been in the habit of employing in the treatment of those eruptions, are the arseniate of ammonia and the arseniate of soda. Both are apt to disorder the stomach, the latter especially so; yet they are sometimes productive of good effects, particularly in the local forms of psoriasis, as in psoriasis palmaria. The arseniate of ammonia I always use first, and generally alternate with it the arseniate of soda. They are best given in solution. The formula I prescribe is somewhat similar to that proposed by Biett, consisting of one grain and a half of the arseniate of ammonia dissolved in three ounces of water, to which are added six drachms of spirit of cinnamon, or of compound spirit of aniseed. The dose of this mixture is one drachm, which contains a twentieth of a grain of the arseniate, and it is gradually increased to two or three drachms three times daily, beyond which dose I do not find it safe or useful to venture. It is best taken in a little plain water, and either during or after meals.

In addition to the use of internal remedies, I always employ external applications in the treatment of psoriasis palmaria, and of the other local forms of the eruption. Iodide of sulphur, in the form of ointment, I have found productive of the best effects, but it must be used very much diluted in the first instance, and the strength of the ointment gradually increased. Not more than eight grains to the ounce of wax cerate should be prescribed in the commencement of the treatment, and the proportion should never be greater than twenty grains to the ounce. The parts affected should be washed, at least three times daily, with a weak solution of the carbonate of soda, half a drachm to a pint of distilled water, and well dried afterwards, previous to the renewed application of the ointment.

As regards pityriasis, when it affects the body generally I treat it, if of short standing, as a mildly inflammatory affection, prescribing gentle saline purgatives twice or three times a week; an alkaline bath,—four ounces of carbonate of soda in sufficient fresh water for an ordinary bath, at the temperature of 80° to 92° F. according to the season of the year,—every second day, and in strong constitutions an occasional bleeding, either general or by the application of half a dozen leeches to the verge of the anus. But when the disease becomes chronic, I use the same treatment as I have above described for psoriasis; it must be remembered that the alkaline bath is not so useful in the chronic as in the acute stage of the affection. The local forms of the disease are, I think, best treated on the plan I have proposed for the eruption when it affects the scalp. The chief additional observation my subsequent experience enables me to offer on the local disease is, that it is of a much more inflammatory character in persons from the age of 12 to 35 than in the very young or the old, and consequently that the use of the mildest local stimulant in them will often cause a marked aggravation of the symptoms, and is therefore scarcely, if at all, admissible in its treatment.

I have found pityriasis a more obstinate eruption to treat than psoriasis, not that the effects of remedies are less marked in causing its disappearance, but it is much more apt to return even in months after an apparent cure has been effected. Whatever plan of treatment, therefore, I find successful in removing the disease in any individual case, I direct to be continued for at least a month or six weeks after the scaly secretion has ceased, and in every case the use of fresh water tepid baths should be directed twice or three times weekly for several months afterwards.

Dublin Quarterly Journal, Nov. 1849, *p.* 345.

179.—CASE OF CANCER OF THE TONGUE.

By Dr. W. Keith, Surgeon to the Aberdeen Royal Infirmary.

Mrs. M'Hardy, aged sixty-six, from the Cabrach, of a pale complexion, aged expression of countenance, and slightly cachectic look, admitted July 3d, 1849, with a large fungus occupying the posterior two-thirds of the tongue on the right side, ragged and ulcerous, and bleeding on the slightest touch.

The fungus commences behind in the base of the tongue, just where the anterior column of the velum-palati touches it. In breadth it all but reaches the mesial line of the tongue, and terminates anteriorly at a point distant half an inch from the centre of the tip.

Is afflicted with constant pain of a burning character in the tumour, aggravated by frequent and severe pains of a lancinating kind, darting through the ear, and extending over the whole right side of the head.

Is almost starved, owing to the presence of the fungus rendering it difficult to swallow fluids, and impossible to get solids over at all.

States that four years since she became aware one night of a warty excrescence on her tongue, her attention having been drawn to it by feeling a taste in her mouth to which she had not previously been accustomed. A succession of these warty elevations, white to look at, arose from time to time around the first one. Until six months since the progress was comparatively slow; the whole then became massed into one fungoid tumour, which enlarged from week to week sensibly.

More recently it has advanced with still greater rapidity, so that the growth of the past three weeks has equalled that of three preceding months.

She has been afflicted with pain from the very commencement of the disease—of a burning and lancinating character—shooting out at the right ear; and her sufferings have all along been much aggravated by any attempt to take food—which, on many occasions, had also the effect of causing considerable hemorrhage.

In the course of the previous treatment she had been subjected to the influence of mercury to the extent of salivation, with no advantage to the disease whatever.

The only topical remedy employed had been lunar caustic, which had been freely and frequently applied to the fungus; the effect was in every instance to aggravate both the disease and her sufferings.

The most careful examination of the neck can detect but a slight thickening of one absorbent gland below the angle of the jaw, and in the course of the external carotid.

Having but one tooth on the right side of her mouth, and that the last molar in the lower jaw, it was at once extracted, to allow of free access to ligature the vessels when the disease came to be excised.

July 20th.—For seventeen days past she has been richly nourished with soups and wine, and was this day brought to the operation chair.

1st. A strong ligature was passed through the tip of the tongue rather to the left side, looped, and given to an assistant, with orders to draw the tongue well out, and towards the left side, at same time instructed to watch closely, and as he saw occasion, to allow her to swallow, should a gulp of blood require her to make the effort.

2nd. Her head was steadied against the breast of another assistant, who had charge of both carotids, with orders to limit the pressure to the vessel on the right side, unless he should receive instructions in the progress of the operation to impede the circulation in both.

3rd. The jaws were widely separated by a wedge of cork entered between the teeth on the left side.

4th. The diseased mass was then firmly grasped between the claws of a vulsellum held in the operator's left hand, and the right half of the tongue swept out by one cut of a strong curved probe-pointed scissors, the diseased mass being thereby wholly and at once removed. The dorsal and sublingual branches of the lingual artery bled with great energy for a few seconds owing to a movement of the patient having altered the position of the right carotid, but both the bleeding vessels were speedily secured and easily ligatured, when all hemorrhage ceased.

5th. To eradicate any trace of disease that might exist by possibility in the tissues contiguous, the actual cautery was then run over the whole surface of the wound; the cavity was then filled with amadou soaked with tincture of matico, and the patient removed to bed. She bore the operation well, and neither moaned nor winced during the whole proceeding. About two hours after the operation hemorrhage took place, which was, however, easily controlled by a finger in the mouth until the operator arrived, who, having emptied the mouth of amadou and a clot of blood, saw, seized, and secured by ligature the dorsal artery previously tied, when the bleeding wholly ceased. The cautery had evidently come in contact with the previous ligature and burnt it.

8 P.M.—The wound has been left open, and under constant and easy inspection; not an ooze of blood has occurred since the dorsal artery was again tied.

[The patient went on very well, and was discharged quite cured on the 17th of August. She had a long journey to go, and travelling some distance in an open cart, she was seized with inflammation of the chest, of which she died in a few days. On this case Dr. Keith makes the following]

Remarks.—There seems to be a dread at the performance of this operation on the part of some surgeons, for which there are no just grounds; while others profess to decline the operation on account of the uncertainty of the cure being permanent. To the former I would only suggest, that choking by hemorrhage appears to me to be the only risk; the mere loss of blood need never excite apprehension. The head should be firmly lodged in a perch, such as is used on a dentist's chair, an able assistant can then moderate the bleeding by compressing the carotid of the side on which the operation is proceeding (and the disease is almost invariably confined to one side); a few seconds suffice for securing the two, or at most three, vessels that are there to be found; and during this short period a right posture of the head, with the remains of the tongue well drawn forward, alike facilitate the ligation of the arteries, and the escape of the blood over the patient's chin and breast. For taking up the vessels the spring or bull-dog forceps is far preferable to the tenaculum—its grasp is decided—its hold is secure and independent of support, so that the operator can dispense with one assistant by dropping the forceps and then running home the ligature for

himself. The ooze afterwards is trifling; the wound left open, and the mouth empty, would afford the greatest security against after bleeding, by allowing it to be at once seen—and seen to. The wound is most favourably circumstanced for healing, and may accordingly be expected to give no further trouble after the first day.

I would never again apply the cautery until after the ligatures had dropped off, when, if anything suspicious appeared in or about the wound, I would assuredly use it most freely as a likely means of destroying any remains of infected tissue that might have escaped excision, and as well calculated, by occasioning the formation of the dense cicatrix so well known after severe burning, to resist the effusion of a blastema bed for cancer cells to become developed in.

To such surgeons as deny to a patient the chance of cure afforded by excision in any case or stage of cancer, I would earnestly commend to your careful perusal the inestimable work on this subject recently given to the profession and to science by Professor Bennett, of Edinburgh. After reasoning the question of the "Rational Treatment of Cancerous and Cancroid Growths" in the most able manner—see page 236, *et seq.*, of his work above referred to—Dr. Bennett at page 245. lays down the practical rule—a rule in the propriety of which I entirely acquiesce—"as based alike on pathology and experience"—"That so long as a cancer remains fixed in a part incapable of being removed, and the strength of the patient is not too much reduced, so long is the surgeon warranted to interfere."

The malignant character of the growth excised from Mrs. M'Hardy's tongue may be assumed from the description I annex, penned after a careful microscopic examination made by our eminent histologist, Dr. Redfern.

"The cut surface of the tumour has healthy muscular substance every where, except near the middle, at which point it partakes of the structure of the whole mass.

"The mass contains scales of epithelium of all forms and sizes, naked nuclei, numbers of fusiform, caudate, and irregular nucleated cells of all sizes, and a number of beautiful spherical and large cells, with large nuclei, and one or two nucleoli."

Monthly Journal, Nov. 1849, *p.* 1213.

180.—ON THE TREATMENT OF CHOLERA.

[It would be a useless waste of time to attempt to analyse all that has been written about cholera during the last six months. Like every other journalist, we have felt ourselves sorely puzzled amongst the various and contradictory opinions, which have been pushed into public, as well as into professional notice. We have, therefore, decided to make a collection of what we have considered the best opinions on the various modes of *treatment*, which we here subjoin in lengthened detail, leaving a shorter abstract of those opinions to be concentrated in the Synopsis at the end of the volume, thus enabling the reader either to take a bird's-eye view in a few moments or to spend an hour on the different papers which we have taken pains to collect from the journals.]

Dr. Stevens' *Saline Treatment.*

[It appears that there is evidence to show that of 1000 cases of cholera treated by Dr. Stevens' method, not more than 6 per cent. have proved fatal. It has, besides, proved extremely successful in Norway, as stated in a letter from the Regent of that country, and also in Sweden, where it has been by far more useful than any method tried. Dr. Stevens gives the following statement of his method of treatment.]

Premonitory symptoms.—Acting under the belief that the premonitory bowel complaint is an effort of nature to expel the poison from the blood and the body, on the first appearance of the premonitory symptoms, a seidlitz powder is to be administered; but if the patient experiences a sense of sinking without diarrhœa, more active saline purgatives must be employed. The aperient I have actually used is from one to three drachms of sulphate of magnesia, added to the seidlitz, and the whole taken in a state of effervescence: when this commences to act on the bowels, the patient is to be encouraged to drink freely of thin beef-tea, well seasoned with common salt. If vomiting is present, a sinapism is to be applied to the epigastrium: if the thirst is intense, seltzer, soda, or plain water, is to be allowed *ad libitum.*

"*Second Stage, or that of Developed Cholera.*—The diagnostic symptoms being cramps, coldness, or sinking prostration. The following powder,

℞. Sodæ sesquicarb., ℈j.; sodii chlorid., ʒj.; potasse chloratis, gr. vii. Misce.

is to be dissolved in half a tumbler of water, and administered in severe cases every half hour; in some malignant cases, every fifteen minutes; while in those cases which are not very severe, it is to be given every hour. The frequency with which the dose is to be repeated in each particular case varies according to its circumstances. In every instance, the saline must be continued until the circulation is fairly restored; when once that point is gained, the intervals between the doses may be lengthened; when re-action is completely established, it is to be left off by degrees. In extreme cases, the dose of the chloride of sodium is to be increased to two drachms, and in some cases to even more than this. In those cases where the stomach is very irritable, a dilute solution of chloride of sodium is to thrown up into the intestines; the temperature of this saline enema being as high as the patient can easily bear, which, as a general rule, is about 100 degrees Fahrenheit. When properly used, this is a means of great value.

"When the stomach is very irritable, the use of the saline powder may be occasionally suspended, and common effervescing mixtures, or small doses of the common soda powders, with an excess of the carbonates, used, until the vomiting abates, and then the carbonate of soda, with larger doses of the chlorate of potash, may be given without the chloride of sodium, or the chlorate of potash may be given by itself, in doses of ten grains each.

"A large mustard poultice is to be applied in such cases to the epigastric region, the moment the patient comes under treatment. When the saline powders are used at the time the stomach is very irritable, it is deemed advisable to dissolve them in a very small quantity of water.

"When cramps supervene, the extremities must be rubbed with hot flannel. The pain produced by the spasms in the muscles is not only relieved by the frictions, but by this, and the application of sinapisms to various parts of the body, the temperature is increased; an object of no trifling importance in the treatment of cholera. In this stage of the disease, as in the premonitory symptoms, seltzer, or cold water, may be allowed the patients, *ad libitum*. A strong infusion of green tea may also occasionally be used in severe cases with advantage.

"The patients must, invariably, be placed in a room, in which a large fire is kept day and night.

"The ejections, and all other impurities, must be removed immediately from the patient's room. No solid food must be allowed the patients for, at least, five days after their recovery from a state of collapse.

"In extremely malignant cases, or when the patient has not been seen till late in the disease, and especially where neither

vomiting nor purging are present, the stage of collapse being well marked, more active measures must be adopted. An ounce of the chloride of sodium, with half a drachm of the chlorate of potash or chloride of potassium, is immediately to be given in cold water, and repeated, if necessary, every half hour until the patient has taken about three doses of this strong solution. If re-action ensues, it is to be kept up by the common saline powders; and if the strong saline given by the mouth fails, as a last resource, a saline fluid must be injected either into the intestines or into the veins."

To the above, Dr. Stevens has appended the following remarks, to which we earnestly call the attention of our readers:—

"In no case," says he, "left to my care was the smallest quantity of opium or other destructive agent administered, until after the cholera poison was evacuated from the system; above all, not in the early stage; for, from what I have seen, I consider this narcotic poison to be as fatal in cholera as it is in either African typhus, or the seasoning fever of the West Indies; and I would urge on those interested, that if they would test the efficacy of the saline treatment, they must have sufficient confidence to trust to it alone, for to poison with opium, or cajeput oil, or to exhaust his nervous system with galvanic shocks, to depress the vital powers with extreme cold, and then, because at the same time with these deleterious agents, salines have been administered, to declare the patient has fallen a victim to cholera, while under the treatment recommended by me, is not only to offer a serious impediment to the spread of a most momentous truth, but to propagate a lie. For results obtained by such a mode of treatment I am not responsible, nor can I allow, without entering my most earnest protest, such cases to be admitted as evidence in determining the value of the non-purgative salines as remedial agents in the treatment of cholera."

Medical Times, Sept. 1, 1849, *p.* 184.

[The Editor of the MEDICAL TIMES states,]

We have obtained a verified copy of the records at Coldbath-fields Prison, relative to the cholera of 1832 and 1833; and a strict examination of this document has completely removed from our minds every shadow of doubt with respect to the truth of the statement originally made by Dr. Stevens, and more recently by Dr. Turley, namely, *that the saline treatment as recommended by Dr. Stevens is actually the most successful of any that has yet been suggested.*

Medical Times, June 16, 1849, *p.* 643.

[The following account of the introduction of Dr. Stevens' saline treatment into Greville-street Hospital is interesting:]

The following statement contains the true history of the introduction of the saline treatment into the Free Hospital, in Greville-street, in 1832. About the middle of May, and after the first

irruption of the cholera at the Coldbath-fields Prison had ceased, one of the governors of the Greville-street Hospital, having heard of the successful treatment of cholera that had lately been employed at the prison, by means of which the medical attendants there had saved nineteen out of every twenty cases, desired to see it tried in the above hospital. He took great pains to ascertain the truth of the reports, and, having convinced himself of their accuracy, he consulted some other governors of the hospital; this conference ended in a firm determination that the cholera patients admitted there should receive the advantage of this new treatment. Mr. Marsden was then consulted; he asserted, that the saline treatment was all quackery, and refused to have anything to do with such empirical trash. At this period there resided near to both the prison and Greville-street Hospital, Mr. Whitmore, a medical gentleman, who having been almost a daily visitor at the prison, was so convinced of the efficacy of the new treatment, that he employed it successfully in his private practice. Mr. Marsden having refused to adopt such quackery and trash, Mr. Whitmore was requested to give a fair trial in their Hospital to the new practice. Mr. Whitmore consulted Dr. Stevens on the subject, and then agreed to treat the first twelve cases, provided that the medical attendants of the Hospital would give him their free consent. By the interference of the governors this condition was agreed to; and Mr. Whitmore having received a promise from Dr. Stevens that he would assist, he willingly undertook the task. An hour was agreed upon, after which the first cases admitted were to be subjected to the saline treatment. The experiment commenced on the 20th of May, and such was the interest the governor felt in the result, that he left an order that he should be sent for on the first case coming in, at whatever hour of the night it might happen.

At what hour of the day the first patient was admitted is doubtful; it appears, however, that Mr. Marsden instead of sending for Mr. Whitmore and the governor when the patient was first presented, made his own experiments on the case, and only sent for Mr. Whitmore late in the night, and after his own treatment had failed. The saline treatment, therefore, was not used in this case, until Mr. Marsden considered the patient to be in a hopeless condition.

The case was a very severe one, and Mr. Marsden admitted that it would not recover under any mode of treatment he had seen tried. Mr. Whitmore said that bad as it was, he had seen worse cases recover under the new plan, not only in the prison, but also in his private practice. The saline treatment was then commenced, and in less than three hours the patient had so far rallied, that Mr. Whitmore pronounced him to be almost out of danger. This was, I believe, the first cholera patient who had rallied from a state of such complete collapse in that establishment,—a most welcome circumstance to the kind heart of the governor, who had so much interested himself in trying to counteract the sad fatality of this disease. Even Mr. Marsden admitted he had never seen so mirac-

ulous a change from any known treatment of cholera, and that he had now before him a strong fact in favour of the new plan of treating cholera, and facts are

"Chiels which winna ding,
And downa be disputed."

Early on the following morning, Mr. Whitmore sent a note to Dr. Stevens to the effect that he had been called by Mr. Marsden, the surgeon to the Institution, and a governor, Mr. Claines, to superintend a case, under the same treatment which had succeeded in the prison.

Immediately after the receipt of this note, Dr. Stevens went to Mr. Whitmore, and they proceeded together to the Hospital, where the patient was doing well; the symptoms of collapse had entirely vanished, without leaving any consecutive fever or bad symptoms of any kind. From this time both Dr. Stevens and Mr. Whitmore attended every fresh case that was admitted into the Hospital. They came in but slowly, but most of them were of the worst description; still, under their treatment, everything progressed favorably. On the 25th inst., three female patients were admitted, all exhibiting the severer symptoms; they were all subjected to the same treatment, and all soon recovered from the state of collapse. Indeed, so energetic was the treatment, and so successful the result, that by ten o'clock on the morning of the 30th, the remaining cases were all believed to be out of danger. Mr. Marsden had visited these and the other cholera patients early that morning, and from what he had already witnessed, no doubt now existed on his mind that the new saline treatment in cholera, was a most important discovery. According to the original agreement the saline treatment was to be put to the test in twelve cases; it had now been tried in ten, of which only one patient perished, a case affording little or no hope when admitted. There was now every reason to believe that the remaining two cases would also recover, leaving a mortality of one death and eleven recoveries. After Dr. Stevens, Mr. Whitmore, and two other medical gentlemen had examined the above cases, Mr. Marsden entered the ward, and addressing himself to Dr. Stevens, made the following observations in a very impressive manner, as if he felt sincerely the truth of every word he uttered. He said, "Dr. Stevens, I candidly confess that before I had seen this new treatment of yours fairly put to the test of experiment, I not only thought but spoke of it with the most thorough contempt; but now, after having seen it tested in so many severe cases, I frankly admit that even you yourself are not more convinced of the worth of the saline treatment in cholera than I now am, and such being the case, if you and Mr. Whitmore will leave the management of the cholera cases to me, I give you my solemn promise, that in future every case of cholera admitted into this Hospital shall be treated in the same manner, and I pledge you my honour, that at the cessation of the present epidemic, the result shall be made known, not only to yourself, but to the public." All this

being said in a way that satisfied Dr. Stevens that Mr. Marsden was serious, he exerted his influence with Mr. Whitmore to cede over the treatment of the future cases to him who was truly the legitimate medical attendant at the Hospital. So long as Dr. Stevens remained in England, no man could act more uprightly than did Mr. Marsden. In a letter, dated October 10, 1832, to the editor of the Medical Gazette, giving the results of the treatment at Greville-street Hospital, he says, "It is right to remark, that the saline treatment in this Hospital was first adopted by Mr. Whitmore, of Cold-bath-square, in a patient that was abandoned by myself as a hopeless case—although this person recovered, and by the like plan, many lives were prolonged, yet no uniform success ensued until the saline powders were faithfully administered every fifteen minutes, night and day, with an unlimited quantity of cold water. Hot saline baths were an important auxiliary, in fact they never failed in relieving the cramps and exciting the extreme circulation, and thereby diminishing the lividity of the extremities. In every attack of the disease, that was not followed by purging and vomiting, all remedies were found perfectly useless, as such individuals died generally within six, never exceeding eight hours from the seizure, which, to me, affords ample illustration of the important effects of vomiting and purging in the collapsed stage of complaint, or even in the early stages, when the function of the liver is arrested. It is worthy of remark, that out of 13 patients, from the age of 10 to 20 years, only one was lost, and this died after a long struggle, having been injected nine or ten times with the saline fluid, rallying more or less after each addition to the venous fluid, the pulse being perceptible at the moment of death. In the 315 cases, there was not one who had bilious diarrhœa. From the above facts, it is quite evident, that until the saline medicines, hot saline baths, and water without limitation, was steadily and faithfully administered, almost every case, in the second stage of the disease, terminated fatally; while, on the other hand, nearly every patient who had not been previously treated by other remedies, but with whom the saline only had been pursued, almost every one survived, as the preceeding report sets forth (out of 81 cases in a state of collapse there were only 7 deaths). With the above facts before me, I must be not only blind to justice, but to all sense of honour and candour, were I not publicly to acknowledge, that until this treatment, suggested by Dr. Stevens, was forced upon my notice by stubborn facts, I not only treated it with disdain, but with determined opposition, yet I feel that I have been amply repaid, inasmuch as I have been taught a salutary lesson, never again to treat with indifference the suggestions of any man, without due reflection and careful investigation. Notwithstanding all the conflicting opinions that have appeared respecting Dr. Stevens' hypothesis, whether true or false, I care not; but I do feel, that to him alone is the profession and the public indebted for the path that he has opened, the beacon that he has lighted, in the vast labyrinth of humoral diseases—for the new era that has commenced in both the pathological and the-

rapeutical sciences—and for having thus cleared the threshold and opened the gates, from whence we view the distant light glimmering over the darkness of past ages, yet sufficiently bright to stimulate every scientific inquirer to a thorough cultivation of this unknown portion of human diseases."

Medical Times, May 26, 1849, *p.* 594.

Saline Injections.

[Mr. H. Howlett relates the case of a lad aged 15, who was in the state of collapse when visited. Mr. H. says:]

Having on the Sunday previous witnessed the marvellous effects produced by saline injection into the veins, I determined without a moment's hesitation, to employ this remedy; but not having been aware, till I arrived, that it was a case of cholera, some valuable time was lost in procuring assistance and the necessary apparatus. I called up my friend, Mr. Girdwood, who immediately accompanied me, and on reaching the patient, found the icy coldness of the body more decided, and the collapse more complete, and in my opinion he was decidedly moribund. We immediately opened a vein in the arm, and injected a quart of water at 98° Fahr., in which one drachm of common salt, and half a drachm of sulphate of potash were dissolved. In less than ten minutes our patient showed symptoms of resuscitation: the pulse returned, the brow became warm, the blue and corrugated skin resumed its natural appearance, and as soon as the warmth became more general, and the pulse was fully re-established, we enveloped him in a sheet wrung out in cold water, and covered with several layers of blankets; hot bottles were applied to the pit of the stomach and calves of the legs and feet; a profuse perspiration was the speedy result. Allowing this to continue for upwards of an hour, we removed the sheet, and placed our patient in bed, enveloped in hot blankets. His pulse was then 120. quite firm, his skin warm and moist, his reason completely restored, and on his expressing a wish to sleep, we left him.

On my visit in the morning, I was delighted to find him in every respect better. Some sickness and fever existed, but were speedily arrested by small doses of calomel and opium. Each day since, he has gone on improving, and he is now convalescent.

This case but confirms me in the opinion I have long entertained, that saline injection into the veins, if adopted before the patient reaches too collapsed a state, is the most rational treatment, and one which, I feel assured, if a fair trial were given it, would be crowned with abundant success. It narrows itself, in my opinion, into a very small compass—demand and supply. The blood is generally deprived with intense rapidity of its serum, so as to leave it in such an inspissated state as to be, in a case I saw lately, thicker than treacle. What, then, so rational as to supply it with its fluid properties again, and that immediately? Not by the slow process of absorption, which must of course be the more tardy the greater the

collapse, but by introducing it at once into the circulation. I would urge my medical brethren to try the usual modes of treatment, particularly that of Dr. Ayre, in the first stages of the disease; should they fail, however, in arresting its progress, and collapse set in, not to lose a moment in employing venous injection, and the result, I am fully persuaded, will be the rapid diminution of deaths from that most fearful scourge, the cholera.

Lancet, Sept. 8, 1849, *p.* 269.

[The following case, which terminated fatally after the use of saline injections, occurred in the practice of Dr. MARSDEN, surgeon to the Royal Free Hospital, London, and is reported by T. C. JACKSON, Esq.]

Miss B——, aged fifty-five, of a spare habit of body, was seen by Dr. Marsden on the morning of the 25th of July, having been suffering for nearly a month from diarrhœa. When seen by Dr. Marsden, the symptoms of true Asiatic cholera were rapidly becoming developed: she had frequent vomiting, extreme prostration of strength, and several evacuations had been passed, at first copious, but afterwards more sparing, highly characteristic of the disorder, consisting of the peculiar rice-water fluid, perfectly serous, and containing flocculent matter in considerable quantity; there was urgent thirst; the pulse thready, and but barely perceptible. Dr. Marsden prescribed for her some powders, consisting of common salt, two drachms; carbonate of soda, one scruple; and chlorate of potash, seven grains: one of these to be taken every quarter of an hour, and as much cold water as could be taken. The collapse, however, increased, and when seen in the evening, at nine P.M., she lay to all appearance at the point of death. She was breathing heavily, labouring, and with a loud noise; the feet and hands were of a deep-mottled purple colour, and the skin covering them was corrugated, and imparted an icy coldness to the touch; the face was shrunken, the eyes were deeply sunk in the orbits and surrounded by a dark zone, the lids half open, and cornea having a peculiar glazed appearance; the mouth wide open; the lips dark and blue; the breath cold to the hand: the countenance shrunken, and the features so pinched that the whole expression was entirely altered. She had taken in all seven of the powders prescribed, but had vomited all but the last; nothing had been taken for the last three hours, as the power of swallowing was wholly gone; she was quite insensible, and could not be roused.

Dr. Marsden now determined to employ the saline injection into the veins, at the same time stating that the case was a very unfavourable one, from the circumstance of the patient having been so much reduced by the long-continued previous diarrhœa. Having dissolved a powder containing common salt, three drachms; carbonate of soda, half a drachm; chlorate of potash, fifteen grains; in forty-eight ounces of water: an incision was made over the median vein, which exposed it; it was then isolated from its cellular connexions, by passing a probe beneath it, and an incision made

into it, and the canula of the injecting apparatus inserted; no movement or expression of pain on the part of the patient attended this operation. The injection was then begun slowly and carefully, and continued until forty ounces of the fluid had been thrown in; about a quarter of an hour was allowed to elapse for the purpose of watching its effects. The pulse at the wrist now gradually became evident, and the skin warm, the corrugation and blue colour rapidly and entirely disappearing. The pulse could be counted, 140 in the minute, and shortly it fell to 120, and became firmer and more regular in its beats. Dr. Marsden, addressing her in a loud voice, requested to know whether she would like anything? She answered, some tea, which being brought, and some brandy mixed with it, she, with a very little assistance, sat up and partook of, and stated that there was brandy in it, that the tea was very weak, but that it was much better than the salt-and-water given to her before. She was not conscious of an operation having been performed. Twenty more ounces of fluid were injected, making, in all, sixty ounces. At half-past ten P.M., being an hour and a half from the commencement of the injection, the pulse was 130, firm, and regular; respiration 46, and free. The surface of the body, hands, and extremities warm, and the general appearance of the patient presenting a strong contrast to that previous to the operation; there was perfect consciousness and considerable re-animation. She expressed a desire to rest. A draught, containing two ounces of the compound tincture of bark, and thirty drops of the tincture of opium, with some water, was then given. This favourable change, however, lasted but three hours. On visiting her at one A.M., she was beginning to sink into an insensible condition; she had not again vomited or purged, neither did the blue colour or corrugation of the skin return; the breathing became hurried, with mucous rhonchus, the pulse gradually failed, and she died at three A.M. on the 26th.

I am indebted to the kindness of Dr. Marsden, in whose practice the above case occurred, for an opportunity of witnessing the operation of injection and its effects. I must confess that, from previous reading and consideration of this subject, there was much doubt in my own mind as to any benefit that could be derived from such a procedure to a patient in so advanced a stage of cholera, more especially in the one now reported. The effect, however, was so very marked, and the patient's apparent restoration so perfect for a limited period, that there seemed to me to be every chance of permanent restoration, had the patient been in a moderate state of previous health, and not so reduced by long-continued disease. It is with a view of drawing attention to the subject of saline injection that the above case is given, in the hope that it may again be thought worthy of trial and investigation, and some useful result obtained, applicable to the relief of such hopeless cases as the majority of those in the collapsed stage of cholera prove to be.

Lancet, Aug. 11, 1849, *p.* 144.

Treatment at GUY'S HOSPITAL.

[In the neighbourhood of Guy's Hospital (at Bermondsey and Rotherhithe) a choleroid affection, probably identical with the *cholerine* of continental writers, has been very prevalent, and even fatal. The best remedy for this affection has been opium, "given judiciously, but fearlessly." Logwood has also been useful.]

In children, in whom it has prevailed most remarkably, and of which there have been twenty to thirty cases at a time in the children's *clinique*, the following has been most efficacious:—Extract of logwood, two scruples; ipecacuanha wine, one drachm; wine of opium, twenty minims; cinnamon water, four ounces. Of this mixture, one drachm to a child of six months; two drachms to a child of one year; and half an ounce to a child of two years,—repeated after every motion; feeding the child on milk, mixed with a little lime-water; keeping it in bed, and covering the abdomen with a hot linseed-meal poultice. This has been highly successful; and under it comparatively few have been lost.

[The results of the experience of Guy's Hospital in true cholera, are these:]

The following facts appear evident:—

1. Not to give stimulants (alcoholic) recklessly, nor to measure the amount to be given by the intensity of collapse. Those patients have most unquestionably done the best who have taken a *minimum* of brandy.

2. Of all stimulants, patients never respond to any so effectually as to chloroform, in doses of ten minims, frequently repeated. In every case in which it has been given, it has comforted the patient, often brought back the pulse to the wrist, and patients have died with ease who would otherwise have suffered intense agony.

3. When the collapse is at hand, but especially when it has come on, *avoid any quantity of opium;* it does no good, and enough has been often given to poison patients if they outlive the collapse. It kills them in the stage of consecutive fever, increasing the delirium and excitement.

4. Most decided benefit has followed the hydropathic practice; at, least, so far as covering the limbs with cloths wrung out of warm water, and covering the patient up with blankets. The cold death-like sweat has often been then exchanged for a warmer excretion from the skin, to the manifest relief of the sufferer.

5. The free use of copious diluents may always be permitted with advantage.

6. *Small* doses of calomel and opium have been chiefly trusted to; but the general feeling is, that the less done, when collapse comes on, the better.

7. In decided collapse, an emetic has sometimes marvellously roused the patient. But the great thing, after all, is to see the patient in the stage of *early cramps*, with collapse, and stop them with opium in the way above described. Cases have been observed in

which there has scarcely been purging, and no vomiting after the first appearance of approaching collapse.

As to the essence of the disease, the writer's own notion is, that cholera depends essentially on a poison, subject to laws *quoad* contagion, like that of common fever; that this enters the blood, and instead, like fever, of tending to the skin, and then producing the peculiar eruption of that disease, it produces a rapid exosmosis of the serous portion of the blood through the capillaries of the mucous membrane of the stomach and intestines. And if the patient does not die from exhaustion from this loss, or from arrest of circulation from draining off of fluids, he eventually sinks poisoned from retained excretions in the blood, just as a person dies in Bright's disease.

Lancet, Aug. 11, 1849, *p.* 154.

Treatment at St. Bartholomew's Hospital.

[Of the various modes of treatment which have been tried at St. Bartholomew's Hospital,]

That which seems to have been attended with most success is the exhibition of an *emetic* of sulphate of zinc, repeated quickly; if not followed by reaction, a warm bath, mustard cataplasms to the epigastrium, and, an hour after the emetic, an effervescing solution of the citrate of soda, has been given at intervals of two or three hours, ice and iced water being administered to allay the thirst. Stimulants and ammonia have had scarcely any effect in rallying patients. Of all methods of exhibiting remedies, that by enemata has been the most beneficial. Injections of beef-tea, with brandy or without, have been used, now, in most of the patients, and with the effect of rallying them considerably.

Aromatics and astringents, gallic acid, tannin, &c., cajeput oil, have all been exhibited, but with no advantage. The most promising plan is the emetic just alluded to.

Lancet, Aug. 11, 1849, *p.* 155.

Treatment at St. Thomas's Hospital.

[Several cases in the stage of collapse have been satisfactorily treated as follows:]

Let a sheet be wrung out in *boiling* water as quickly as possible, then envelope the patient in it completely, only leaving out the head; cover the sheet over with eight or ten blankets, keep the patient in it for an hour, by which time reaction and profuse perspiration have generally taken place. The patient is then to be put into a clean warm bed; a little weak brandy-and-water may be given at first, to assist the circulation, after a very short time, whilst in the sheet. Nitro-hydrochloric acid, and syrup of poppies, have been given, either in water or infusion of catechu. If the stomach will bear it, the acid is repeated every hour or oftener, according to circumstances. So many cases of consecutive fever

have followed the exhibition of opium, that very great caution about using it is adopted at St. Thomas's. A case of very violent diarrhœa has also yielded, when nothing else would remain on the stomach, to the nitro-hydrochloric acid.

Lancet, Aug. 11, 1849, *p.* 155.

Treatment by Salines and Camphor, &c.

[Dr. James Copland, F.R.S., says]

Amongst the means which I have employed during the present outbreak of choleric pestilence, I have found, during the stage of collapse, the use of the chlorate of potash, with bicarbonate of soda and hydrochloric ether, in camphor water—or the aqua chlorinii, with the same æther, taken in the same vehicle—to be the most successful; but these should be given in small and frequent doses. or largely diluted with the camphor water, or be taken in the patient's drink. They ought, however, to be strenuously persisted in, notwithstanding the frequency of the vomiting. If reaction follow, then they should be relinquished. and calomel and camphor ought to be given to remove the torpor of the liver, and to promote the portal circulation. If the kidneys are inactive in this stage, embrocations, with turpentine, may be placed over the loins, and the diuretic salts be given internally; or an enema, containing a moderate quantity of spirit of turpentine, be administered. If the vomiting continues during the state of reaction, the patient rarely recovers, and never unless urine be secreted.

I must content myself at present with the above hasty remarks, and must only add, that whatever twaddle may appear on the subject of "non-contagion," of "non-infection," of "contingent contagion," of "contingent infection," pestilential cholera will some day be acknowledged to be what I have long ago, and again more recently, endeavoured to show it to be, according to the most irrefragable evidence, a disease *sui generis*, which is propagated by a specific infectious emanation proceeding from the sick, as observed in respect of true typhus and scarlet fevers, whenever and wherever the predisposing and concurring causes are in operation, as fully shown in the articles "*Disease*," "*Infection*," and "*Pestilences*," in my work on Practical Medicine.

Medical Gazette, Sept. 28, 1849, *p.* 543.

Calomel Treatment.

[The following observations are by Dr. Joseph Ayre, of Hull. At the time they were written, little more than twenty-three days after the appearance of the cholera in Hull, there had been eighty-one cases of the disease, more or less advanced into the collapsed stage; and of this number only ten had died. Dr. Ayre's remarks are as follows:]

1st. as to the intensity of the disorder, it might suffice to take, in their order, the first four cases we were officially called to attend, which, in company with Mr. Archbald and my other colleagues, I especially attended, and by which I aimed at, and succeeded in, the

fixing their attention to the mode I followed in the treatment. I gained, by the results of these cases, the entire confidence of my colleagues in its power. The first case was in a girl, (Marshall), nineteen years of age, who resided in St. George's-place, Little Passage-street, and had nursed a sister in the same court, who had died under the opiate and stimulant treatment. This patient (the girl) became pulseless, and the discharges were so copious as to pass through the bed, and flow in a stream along the floor. The secretion of the kidneys was stopped for seventy-four hours, being longer by two hours than had happened to any patient I saw in 1832. This patient took a large quantity of calomel, in two-grain doses, was convalescent, that is to say, was safe from the disease, and free from consecutive fever, and was quite well in eight days. The next patient was also a girl in the same court, (Brewer.) See was thrust out from her lodging in an adjoining house, and, in the absence of its owner, crawled into the house, and got into the bed of a woman who had died of the complaint two days before. She was nearly pulseless, but, like the first patient, was carefully attended by a hired nurse, and was convalescent in two days. The urine was suppressed forty-eight hours. The third case was a youth, (Collins,) aged eighteen, of Duke-street. He had lost his mother a few days before, under the stimulant and opiate treatment, and his sister was dying at the time of our visit, under the same treatment. They had all occupied the same room. The discharges of the patient ran from him, through the bed, and the secretion of the kidneys was suspended sixty-three hours. He took a large quantity of calomel; had very slight ptyalism; was convalescent in three days; and was walking about, in seeming good health, at the end of the week of his attack. This patient was seen in company with Dr. Sutherland. The fourth patient was a married sister of the foregoing, (Smith). She had been several days acting as a nurse to her family, when she was struck down with the disease. She was, however, seen early, and was convalescent in two days, with a slight degree of ptyalism.

But to attain a just knowledge of the intensity of the disease, the interests of truth demand of me to say, that without any means of knowing the number that have been attacked, the mortality from the disease in this town has been very great under the opiate and stimulant treatment, which has been too generally pursued, and, as I will presume, from an ignorance of any better method. As the official attendants of the poor, and having been widely advertised as such, my colleagues and myself have necessarily had the greater number of the patients attacked; and when I notice among our recoveries a woman eight months *enciente*, a half-famished Irish labourer fetched in by the police from the outside of the town in a cold and livid state, a child two years and a half old several hours pulseless,—it may be readily imagined that we have had our full share of intensely affected patients.

But, as I have stated, if the treatment be available for the cure of twenty severe cases, it should, *cæteris paribus*, be available for the

cure of thirty, and so on; and I have, in a former report of my treatment and its results, deemed it right, not only to explain how I saved some, but why I did not save all; for in the absence of any disease previously present, and of the causes now to be noticed, every patient, or well-nigh every patient, might be saved. The causes which have appeared to prevent our entire success in curing all our patients are mainly two—one of these is a most blameable delay on the part of the friends in sending for medical aid. From causes pertaining either to the constitution of the patient, or to the concentrated intensity of the *malaria*, the disease differs greatly in different individuals in the rapidity of its course, varying in the period of its fatal termination from four or five hours to twelve or twenty-four. In too many cases, especially when the disorder commences in the night, assistance is not sought for by the friends until four or five hours have elapsed, when the patient is already literally moribund, and when the stomach has become insensible to the influence of every medicinal agent, and the lungs and brain have become irrecoverably congested, as shown by symptoms the most intelligible to those familiar with the disease. But what, if possible, is worse even than delay, and more palpably a hindrance to success in the treatment by calomel, is when the opiate and stimulant treatment has been already tried upon the patient, and which failing to restore the secretions, increases the congestion, and puts in abeyance those vital sensibilities and sympathies which are needed to render the calomel treatment efficient. Of the ten patients who died, three of whom were children, one was in the disease in its severest forms more than six, and the other about five hours; they were pulseless when visited, and had probably been so for some time. Five had been under treatment by others, and had been drugged with opiates and brandy, and with camphor and cayenne pepper; the ninth was subject to fits of insanity, and when he had considerably recovered from his disease, and had been several days out of collapse, he became suddenly ill, (and if his daughter was to be credited, it was from his having swallowed some hard substances); and the tenth was a drunkard and of a drunken family, and became affected with congestion of the brain, when he had become fully convalescent and had taken some rum. These then, are the patients whom we lost, and the circumstances I have enumerated are the causes which occasioned their loss. It may indeed be the case that the disease is of such intensity as to destroy the vital powers at its first assault; but neither in 1832, nor in the present epidemic, which is to the full as severe in its symptoms as it was then, have I ever met with a case in which, with all the diagnostic symptoms of collapse clearly exhibited, and with a reasonable time afforded to begin it, the treatment I have adopted did not avail to subdue it; and it is with the emphasis and solemnity of truth that I now make this statement, appealing, as I confidently can do, to the experience of my three colleagues in the treatment during the whole time in which we have worked together.

With respect to the treatment pursued by us with calomel, I may here

repeat, (what I have stated at some length in my work on the disease, and in an abridged form in the pages of the Lancet), that *when a patient is entering the stage of collapse, as shown by the blue-colour, vomiting and purging, and weight at the præcordia and cramps, we begin our treatment by giving a mixed pill, containing two grains of calomel, every ten minutes, with one, or at most two, drops of laudanum in a tea or table spoonful of water; we continue the calomel at the same intervals for five, six, or more hours, watching the course of the symptoms, and taking care to reduce the dose and widen the intervals of exhibiting the calomel as the coldness and lividness subside; and when the surface has become of a natural temperature, and the pulse natural, it may be wholly discontinued.* There is no limit required to be set to the time during which it may be given, or the quantity that may be taken. Pending the duration of the stage of collapse there is no absorption of the calomel into the system, and no benefit to be obtained by aiming at such an effect, and although no evil nor even an inconvenience has been sustained by a slight tenderness of the gums, which is sometimes produced, yet it is not to be desired as a remedial means in this disease. A patient, (Higgins, of Mill-street), who was seized on the Sunday, and was walking in the street on the following Saturday quite well, took three hundred and fifty grains of calomel before the collapse was fully removed, and during seventy-seven hours of interrupted urinary secretion, as I have just learnt from Mr. Gibson; and with the exception of a slight tenderness of the gums, and without any flow of saliva, she became and still remains in every respect quite well—I have seen her this morning. And this leads me to remark, regarding the results of this mode of treatment, that the operation of the calomel being to restore the secretion of the liver, and thereby remove the congestion of it and its associated organs, there is no consecutive fever following the collapse, and not one of our recovered patients had any degree of fever, but came at once into a state of comparative health in coming out of their collapse; and although we have had seventy-eight patients in this disease in the space of three weeks, we have never had more than five or six patients under treatment, the rest having recovered, and if women, have followed their various avocations, one patient being in two or three days sufficiently recovered to be able to act as nurse to another of the family struck down by the disease. The occurrence of ptyalism was so rare and so trifling as to make it of no account, although the average quantity of calomel could not be much less than fifty or sixty grains. In giving laudanum my object has *only been to detain the calomel for a time in the stomach*, and not with any view to act as a stimulus to rouse and support the vital powers, or as a sedative to remove the cramps; for I have always held and acted upon the opinion, that the abeyance of the vital powers, as shown in the shrunken features, cold and livid surface, and also the cramps, and, indeed, every one of the appalling symptoms of the complaint, are dependent upon a venous congestion of the liver, produced by its interrupted secretion, and that it is by means that will restore that

secretion that the whole of its symptoms is to be removed. I care not, therefore, how little laudanum is exhibited, or for how short a time it is continued; and would advise that it should be taken only at wide intervals after forty or fifty drops have been given. I still give it because I originally began with it, and because my patients have recovered under its use, but *I am strongly disposed to give the calomel alone*, and have already tried it on an infant with success although the patient was pulseless when the treatment began.

With respect to any other auxiliary means, my practice is, as it ever was, *not to employ any*. To our patients not one drop of brandy or other fermented liquor has been given, nor have we ever done more than sometimes to order a hot bottle to be put to the feet, or a small mustard plaster to the stomach, though, I believe, in nine cases out of ten these means have been left unordered. One difficulty we have had to overcome is *the negligence of the hireling nurses*, and we have often held them to their duty by the promise of a reward if the patient recovered.

And now, having stated as much as it occurs to me can be needed to make the results of our treatment and the nature and object of it intelligible, I shall conclude by re-stating what I would desire to be especially insisted on—namely,

1st. That there is no period in the stage of collapse short of a visibly moribund state that should make us despair of restoring the patient, or induce us to desist from using the treatment.

2nd. Whatever may be the quantity of calomel employed during the period of collapse, no evil of any kind results from its use; and when the patient is restored, the only inconvenience that ever happens is a slight tenderness of the gums, which may generally be prevented by desisting at the proper time from exhibiting the medicine.

3rd. That opiates in excess, and all stimulants, are to be avoided as especially injurious, and tend to produce in the recovered collapsed patient a state of fever, which may itself become a cause of death, but which never takes place under the calomel treatment, as the patient, in coming out of the collapse, returns to a state of comparative health, and is generally able to walk out of doors in three or four days, and take his food as usual.

4th. That besides the injurious effects justly ascribable to opiates in any considerable quantity, and to all stimulants, whether alcoholic or pharmaceutic, there is a general objection to be offered to all agents, auxiliary or otherwise, when employed to obviate this or that symptom, whether it be the cramps or the coldness, for these are but symptoms of an internal morbid condition, and whatever tends to remove that, removes also all the symptoms dependent on that stage, and my early experience in the efficacy of calomel given in small and frequently repeated doses to remove the morbid cause, has taught me to depend upon that single remedy, unmixed with any other means. As neither in the epidemic of 1832, nor in the present one, have I ever had recourse to any other, so have I never had occasion to regret my exclusive employment

of it; and if in the cure of this appalling malady, whose symptoms and course are so unerringly uniform, the calomel in small and constantly repeated doses is not a specific, than is there no remedy that ever merited that name.

Lancet, Aug. 11, 1849, *p.* 145.

[Mr. W. N. Spong, of Feversham, when called to a man in the collapsed stage of cholera, adopted the following treatment.]

To take two grains of calomel and two drops of tincture of opium every ten minutes. This is Dr. Ayre's plan of treatment. In addition, to have a hot-air bath, and take ten drops of chloroform with every alternate dose of the medicine; also, a drachm of strong mercurial ointment to be placed in each axilla; this was rubbed in by the patient himself, in consequence of his frequent spasmodic movements. Twenty-seven drops of chloroform are equal to five minims.

21st.—One A.M.: After six hours' treatment there is decided amendment; the attacks of cramp and vomiting are less severe, and the intervals prolonged. Extreme thirst, but if more than half an ounce of fluid is taken at one time, it is instantly rejected; commencing re-action. Omit the opiate and chloroform. One P.M.; Improving; reaction fully established; the purging has ceased; cramps and vomiting only occasionally recur. To take the calomel every half hour.

22nd.—At six this morning a well-marked crisis took place. The urinary secretion was restored after thirty-eight hours' suppression, the liver performed its function, and a black-coloured motion was passed, followed by several characteristic chopped spinach stools. From this hour he gradually progressed. To take small doses of solution of acetate of ammonia with the alkali slightly in excess, and no other medicine.

27th.—Convalescent.

Remarks.—If you desire a particular remedy to be given, tell the nurse to give it; but if you want it to be given properly, give it yourself. The latter part of this precept was acted upon for the first eight hours, and the medicine punctually given. The readiest plan is, to throw a couple of drachms of calomel on a sheet of paper, and divide and subdivide into sixty parts; one of these for a dose is best taken from the point of a table-knife. Again, with regard to the opiate, bale out any number of spoonfuls of water, add twice the number of drops of laudanum, using this spoon to administer the same; thus accurate quantities are known to be given. A hot-air bath is easily and efficiently given as follows:—Fasten a blanket loosely around the body with large pins, to prevent the patient from kicking it off during the attacks of cramps; place two chairs upon the bed, so that the backs form an arch over the person, tie them side by side, and over these throw several other blankets. A spirit-lamp can now be safely held beneath at the foot for a few minutes, which will suffice to raise the temperature to one hundred and thirty degrees or more. In this disease minutes

are as valuable as hours; if a spirit-lamp is not at hand, an excellent one can be made out of a tea-pot; pour in four or five ounces of spirits of wine, and make a wick by unravelling a piece of calico; collect the threads into a twisted form, pass these down the spout, and the lamp is perfect. The hot-air bath appeared to relieve internal congestion, and the chloroform was given with the intention of mitigating the agonizing cramps. But supposing the question to be fairly put, "What was *the* remedy in this case?"—my answer would be, most decidedly the calomel, given in this particular manner. The patient took two drachms and a half of this, about one drachm of chloroform, and only forty minims of tincture of opium; the gums were but slightly touched. No brandy was at any time given, and to this is in great part attributed the entire absence of consecutive fever. During the collapse a circumstance occurred that deeply affected this man; but I observed, that although he cried, or, more strictly speaking, made a noise, yet he could not shed tears; this secretion in common with all others, wanting its healthy stimulus. At the present moment it behoves every practitioner to be fully prepared in his own mind as to the method of treatment he intends to pursue, when called upon to combat this terrific enemy. If the constitution is not utterly depraved, Dr. Ayre has ably shown that calomel may be depended upon, and with this for a sheet anchor, the frail vessel will often outride the storm.

Lancet, Aug 4, 1849, *p.* 124.

[Dr. J. C. Badeley, of Chelmsford, relates a case in which, when the patient was in a state of complete collapse, the administration of a scruple of calomel every hour, for three times, was followed by complete and rapid recovery.]

Lancet, Sept. 8, 1849, *p.* 269.

[An anonymous correspondent of the Lancet, speaking of the calomel treatment, says:]

In the annexed summary of Dr. Cox's cases, it will be seen that the quantity of calomel administered, and the number of hours elapsing before its effects were visible, are large, in direct proportion to the smallness of the doses given. The three most speedy recoveries were those in which five and three grains were taken every ten minutes; in one case every fifteen minutes; and in these the aggregate is infinitely less than where one-grain or two-grain doses were preferred. Of the two deaths which occurred three or four days after reaction, one patient had taken 480 grains; the other 384 grains, in two-grain doses. In Mr. Pery's case, where twenty-four grains were given in the time that would only have afforded

eight grains, according to the two-grain system, the results appear much more marked and satisfactory; and I can assure your readers that the certainty and rapidity of the effect is incomparably greater where the full dose—twenty or even thirty grains—is given at once. In speaking of this patient, he remarks, as others have done who have given large doses of calomel, that "he felt very well, but exceedingly weak," after a bilious motion was produced. I think there can be no hesitation in deciding by which method this effect will be most speedily obtained. To those who have a doubt, I beg to offer my experience, as most positively in favour of the larger quantity, which seldom requires to be repeated, and never more than once, or, at the most, twice; whilst it is shown that, in what appears to be the mildest treatment, the quantity varies from two drachms to an ounce.

As these gentlemen have not the fear of giving calomel to an unusual extent, they cannot object (as many others do, who have not tried it) to administer the quantity I recommend, on account of its magnitude; and, it must be evident, that one such dose is better calculated to attain the object aimed at—viz. "to act forcibly and speedily upon the stomach, liver, and kidneys, with as little risk as possible of impregnating the system with mercury." Upon the stomach, as I have mentioned in my last letter, a direct sedative effect is obtained, whilst the secretion of the liver and kidneys is rapidly induced. There is this further advantage in the large doses —the medical man can be assured of its being taken. By always having some with him, he can place it on the patient's tongue; and if no vomiting occur within ten minutes, he may rest pretty certain of the effects.

If vomiting should take place within that time, he may give another dose, and leave his patient without anxiety, as even should there be further irritability of the stomach, enough will be retained from the two administrations to check it very speedily, and to produce the other results indicated. He has only to give directions for the auxiliary treatment, such as warmth, friction, effervescing drinks, and gentle stimulants; and remain confident that the calomel is effectually doing its duty, whilst he is attending to other cases.

I feel called upon to protest against the administration of calomel in small doses often repeated, from the assurance that it will lead to disappointment, and probably induce the profession to abandon its use, as was the case before; and however satisfactory it must be to find that it has been so useful, I believe, as I have endeavoured to point out, that these very cases indicate the propriety and superiority of one or two effective doses in the first instance, which seem to have the following advantages: first, more speedy and certain action; secondly, the smaller amount necessary to be given, and consequently the less risk of injuriously affecting the constitution: thirdly, the great advantage of knowing that your remedy is taken, and has a fair chance of doing its duty.

Summary of the cases reported by Mr. Cox.

Recoveries.		Reaction in.	Consumed.
3 most speedy, taking	5 grs. every 10 min.	2 hrs.	60 grs.
	3 grs. every 10 min.	3 hrs.	54 grs.
	3 grs. every 15 min.	3 hrs.	36 grs.
5 taking	2 grs. every 10 min.	10 hrs.	120 grs.
	Ditto ditto	16 hrs.	192 grs.
	Ditto ditto	12 hrs.	144 grs.
	Ditto ditto	6 hrs.	72 grs.
	Ditto ditto	9 hrs.	108 grs.
2 taking	1 gr. every 5 min. each	14 hrs.	168 grs.

8 time previous to rallying not stated.—Total. 18.

Deaths.	Died in.	Having taken.
1	16 hrs.	384 grs.
1	10 hrs.	240 grs.
1 died on the 4th day, after rallying in	16 hrs.	384 grs.
1 died on the 3rd day, after rallying in	20 hrs.	480 grs.

2 Time previous to death not stated.—Total, 6.

Lancet, *Sep.* 1, 1849, *p.* 236.

[Dr. Hughes of Guy's Hospital, does not agree to the efficacy of the calomel treatment. Speaking of the cases which he has seen treated by the administration of small doses of calomel frequently repeated, he says,]

I believe that all, or at any rate almost all, the patients so treated have died. So common, indeed, has been this result, that latterly, when, on entering the sick room, I have seen some dozens of powders upon the dressing table, and have found the patient's tongue coated with white, as from so much chalk or carbonate of lead, I have at length assumed that his fate was almost certainly sealed. I regret to express my belief that I have been very rarely mistaken. Such, Sir, is the result of my own observation of the treatment of cholera by calomel.

[Dr. Hughes states this to be the opinion also of many other medical men with whom he has conversed. Dr. H. thinks that the most important point in the treatment, is to stop the drain from the alimentary canal. He says,]

For the purpose of stopping this drain, I have myself found nothing so effectual as a large dose of solid opium by the mouth; followed by astringents, ammonia, and opium in a fluid form; together with an enema composed of a small quantity of warm starch, and a full dose of laudanum. If these means are employed early in cholera (and by cholera I mean neither diarrhœa, however profuse on the one hand, or the collapse of cholera on the other), I believe that they will frequently be effectual in checking the disease. They have certainly been the most efficient remedies that I have myself employed, or seen employed. When collapse occurs, they must of course be laid aside. Then, indeed, calomel may be given, for then it can do no hurt, though I verily believe it can effect no good: then, so far as I have observed, the free administration of

cold fluids, together with a moderate amount of stimulants in the form of ether, ammonia, chloric ether, or turpentine, sponging the surface of the body, and wrapping it in warm blankets, are almost the only, or at any rate the best means to be adopted for the possible restoration of the patient. *Medical Gazette, Sept.* 21, 1849, *p.* 485.

[At a meeting of the London Medical Society, Dr. Golding Bird said,]

With respect to the calomel treatment—that by small and often-repeated doses—he had seen much of it, both at home and abroad; he had tried it himself, carefully, assiduously, and constantly; and he had come to the conclusion that it was of no benefit whatever. He could only reiterate the opinion of his colleague, Dr. Hughes, on this point. *Medical Gazette, Nov.* 2, 1849, *p.* 769.

Treatment recommended by T. M. Greenhow, Esq. *Newcastle on-Tyne.*

If by the term poison is intended to be expressed matter, whether solid, liquid, or aerial, which being received into the human system, produces morbid actions, threatening death, or suspends vital actions essential to life, then must the physical cause of cholera be properly a poison; and the history of nearly every case of the disease proves that the first impression of this poison is made on the mucous membrane of the intestines, and the first effect is the entire suspension of its functions of absorption and secretion, while an increased or spasmodic action is induced in the muscular coat of the same viscera. Hence the copious and exhausting drainage through the former, and rapid expulsion by the latter coat of the intestinal tube of the thinner portions of the circulating mass of blood; and this is soon assisted by the extension of the muscular spasm to the walls of the abdomen, at the same time that similar diseased actions are extended to the respective coats of the stomach, giving rise to like discharges of serous fluid from both extremities of the digestive canal. So rapid is this drain, that not unfrequently, by rendering the blood viscid in quality, and greatly diminished in quantity, it very soon leads to the entire suspension of secretion, a direct abstraction of the material whence the secreted fluids are to be supplied is effected; and hence the absence of bile in the excretions, and the suppression of urine, by which cholera is characterized; hence, too, the almost entire suspension of the heart's action, and of the arterial and venous circulation, and, as a necessary consequence, the cessation of the oxygenation of the blood, and the formation of animal heat. Though the lungs continue to perform the mechanical function of respiration, the office of vital chemistry ceases, and hence the cold tongue and cold breath, and the external appearances which so remarkably distinguish the most intense forms of the disease.

The morbid effects of the poison on the coats of the intestines, constitute the first and most curable stage of cholera. The second

stage is marked by some, or all, of the subsequent train of symptoms. The third stage is one of partial recovery, marked by symptoms of congestion or inflammation of the brain or other organs, the results of the efforts of nature to restore organic function; not unfrequently, and unfavourably influenced, it is to be feared, by excessive doses of opium, or stimulants given as restoratives during the former stages of the attack.

This hasty sketch of the paroxysm of cholera naturally suggests clear and simple indications for our guidance in its treatment. In the first stage, to check the serous discharges, relieve spasm, and restore secretion, must be our great objects; and mercury and opium are the remedies to be relied upon. But in what doses must they be given? This, indeed, is the great problem to be determined; and as far as our experience goes, we will attempt its solution. Very large doses of either medicine are not needed, and will certainly do harm; in the one case, by increasing, instead of relieving, local muscular spasm; in the other, by confirming the arrest of secretion, and by its subsequent effects, especially on the brain. Without affirming that we are enabled to define precisely the best form and doses, we can at least specify the formula which we have found generally successful in answering the intended indications. To render the narrative of treatment simple and uncomplicated, the prescription is subjoined:—Mercury pill, half a drachm; ginger powder, twelve grains; opium powder, six grains. Mix, and divide into twelve pills; two to be taken immediately, and one every hour while the purging and vomiting continue. Should the first dose of pills be rejected, they had better be soon repeated. After one, two, or more doses of the pills, it generally happens that the diarrhœa of the first stage ceases, or is much mitigated; and their continuance, at longer or shorter intervals, soon removes it entirely.

The diet requires somewhat strict regulation. When the diarrhœa is severe, attended, probably, with griping, solid animal food, fruit, and vegetables, must be entirely abandoned, and salted gruel, or especially chicken-broth, with toasted bread, will agree best. Cold water, iced, is generally much relished, and may be taken pretty freely. When the attack is slight, solid but simple animal food may be allowed; and if the patient is accustomed to stimulants, brandy, in great moderation, in water, at dinner. In a day or two it is probable that a gentle purgative will be needed; in which case castor oil, rhubarb and magnesia, or aloes, either in pills or the compound decoction, will be found most suitable for promoting solid evacuations. But it frequently happens that the pills alone suffice to restore a perfectly healthy action of the bowels. During convalescence it is sometimes useful to give infusion of calumba, with a few grains of carbonate of soda, and a little aromatic tincture twice a day. The return to ordinary habits of diet ought to be gradual, and caution should be observed for some time in general regimen.

The symptoms of the second stage of cholera are of so marked and grave a character as to demand the closest attention. The

prostration of muscular strength is often so considerable as to require the greatest quietude. Warmth and comfort must be promoted, but busy and constant efforts to restore animal heat will certainly do harm, by increasing fatigue, and cannot be successful. This effect can only result from restored function, and cannot be produced by external applications, though, when judiciously employed, they may place the person in a situation favourable for the action of other remedies. He ought therefore to be placed in a warm bed, and sinapisms to the feet and pit of the stomach may do good. Internal stimulants, whether aromatic oils, or alcohol in any form, or ether, are, for reasons stated above, injurious. As medicines adapted to the indications of treatment, the pills of the preceding formula will suffice; two to be given immediately, and one every hour while the serous discharges continue. Should the diarrhœa cease, and the vomiting continue, a few grains of carbonate of magnesia, in mint water, or in a state of effervescence, given occasionally, will often check it successfully. Ordered, carbonate of magnesia, one drachm; mint water, eight ounces; compound tincture of cardamoms, three drachms. Mix. Three tablespoonfuls to be taken every hour or two, according to the urgency of the vomiting. The use of the pills to be continued. Or, carbonate of magnesia, one drachm; carbonate of soda, three drachms; lump sugar, two drachms; water, eight ounces. Mix. Two tablespoonfuls to be taken with one of lemon-juice.

Liquid diet—chicken broth, warm or cold, or gruel, with salt—should be given in unlimited quantities, without regard to the frequency with which it may be rejected by the stomach. It will supply the place of the exuded serum, so as, in the first place, to prevent further waste, and subsequently refill the emptied blood-vessels. The face may be occasionally sponged with water, tepid or cold, as may be found most grateful; and when reaction has fairly commenced, the entire surface of the body should be washed with warm vinegar and water. Such ablution acts favourably on the skin, both as a grateful stimulus, and by removing the perspirable matter, which has often been profuse in quantity, and in quality resembling the other serous discharges.

The patient's room must be kept, throughout the treatment, perfectly airy and well ventilated; and the greatest care must be taken, during the stage of collapse, not to harass or fatigue him by too constant interference. In an hour or two it is probable, even in very bad cases, that some improvement will be observed. The evacuations will be lessened or cease altogether, or the diarrhœa or vomiting only will be arrested—often the former while the latter continues, though mitigated in degree; sometimes the contrary is the case. The cramps will be partially or entirely relieved; the pulse will become more distinct and increased in volume; the tongue and breath less cold; the features and extremities less shrunk, and possibly some refreshing sleep may take place. This condition of things is favourable, but must not be too certainly relied upon. The pills may be taken at longer intervals, the same system of diet

persevered in, and muscular exertion with equal care avoided. The discharge of urine is a very favourable sign. It proves the replacement, in some degree, of the *pabulum* of secretion, and the restoration of secretive power. Sometimes it is several hours before the symptoms give way, or anything like reaction is established. Should no improvement occur, but, on the contrary, the state of the patient become more alarming, the injection into the bowels of one, two, or three pints of the gruel with salt, or chicken broth, will sometimes be found extremely useful. The effect of such injection may be either to supply the place of the serous exudation; by absorption, if reaction is beginning, to replenish the empty vessels; or to soothe and relieve the spasmodic action of the muscular coat of the intestines. But whether by any, or all, of these modes of action, it is certain that sometimes the most salutary effects ensue, leading quickly to the restoration of suspended functions.

When, by these measures, the most formidable symptoms have been subdued; when the vomiting and purging of serous fluid have ceased; when urine begins to be discharged, and the pulse returns or assumes more volume and vigour; the tongue and breath have become warmer, and the cramps or muscular spasms have either ceased or are greatly mitigated; any description of animal broths, with toasted bread, should be given in abundance, varied, perhaps, by the farinacea in common use. Serous discharges from the bowels may still take place, at longer intervals and in smaller quantities, in which case a pill may be taken every three or four hours. This is the time when the power of the mercurial, either already taken or now given, will be successfully exerted in the excitement of the liver, and we shall look with anxiety for the appearance of bile in the stools; such appearance is generally soon followed by the proper fæculent character of these evacuations, accompanied with their characteristic odour; and the best assurance is thus given of convalescence, unless, indeed, the patient should be affected with some of those formidable symptoms which occasionally occur, and constitute the third stage of the disease. The nature of these symptoms has already been referred to. They may have their seat in the head, which frequently happens, especially when opium and stimulants have been given liberally, or in the liver, or other internal organ. Sometimes continued fever ensues, either immediately or in a few days; but whatever may be the nature of the disease which follows cholera, rather as an effect than as a proper or necessary portion of its attack, it must be treated on the general principles of therapeutical science, which need not here be enlarged upon; we may, however, remark that it is in this stage that bleeding, local or general, may be required. In the stage of collapse, a little reflection must convince us of its total unfitness; the vascular system, drained of its more fluid contents, cannot with impunity part with what still remains, and the system, instead of being relieved, sometimes, as we have seen, immediately sinks, from the abstraction of any quantity of thick, tar-like blood that can be squeezed out of the collapsed vein; to this result the fatigue of the operation not a little

contributes. But in the third stage the condition of things is totally changed; the blood has recovered its fluidity and its original quantity—perhaps even in excess; while its distribution throughout the system has not been equalized; and in one or more important viscera it is accumulated in venous congestion, or inflammatory arterial activity.

Lancet, July 28, 1849, *p.* 89.

Treatment by Counter-irritation to the Spine.

[Mr. REEVES, of Carlisle, who refers the symptoms of cholera to some kind of spinal derangement, recommends the employment of counter-irritation over the spine. He says:]

I am tenacious concerning the treatment of cholera by counter-irritation over the spine, for I have found it effective. When the case is urgent, my plan is to rub the whole region of the spine with a solution of croton oil in turpentine—one drachm to the ounce—for twenty minutes, and then apply a hot and moist bran poultice, contained in a bag, so as to be applied with more convenience. The bag containing the bran must be large enough to cover the whole back. This treatment soon shows its good effects; the benefit is astonishing. I have seen all the symptoms cease, or become so materially diminished under its use, as to relieve from anxiety or fear as to the result. In cases of relapse the treatment requires a repetition.

Lancet, Sep. 8. 1849, *p.* 269.

Treatment recommended by Mr. GARRINGTON.

[Mr. GARRINGTON, Medical Officer to the Portsea Island Union, recommends the following plan of treatment:]

A blanket, after being dipped in the hottest water, is wrung out as dry as possible, and when spread out on a mattrass, is sprinkled with a handful or two of salt. The patient (previously stripped) is then put into it, and wrapped round with expedition, the upper part being fastened close round the neck. Four or five dry blankets are then put over the patient, and pressed in at the sides. This has been found to require renewing in an hour and a half or two hours.

I have, since Saturday last, (28th ult.,) treated eleven cases of complete collapse in this manner in the hospital of the Portsea Island Union-house, giving cold water *ad libitum*, and beef-tea frequently, without any medicine, except in a few cases, where a mixture composed of a small quantity of sesquicarbonate of ammonia and soda, and hydrocyanic acid, was given to allay great sickness, but this, even, was withdrawn as soon as possible.

Of these eleven cases, only three have resulted in death, a success much greater than I have obtained after any other procedure. The patients, too, who have been treated by this method, have, in addition to the more speedy reaction, all recovered without the congestions so constantly observed after the stimulating plan, and which are generally acknowledged to be so difficult of management, and so frequently fatal.

Lancet, Aug. 11, 1849, *p.* 150.

Treatment by Phosphorus.

[Mr. W. Batten, of Pimlico, states that in the epidemic cholera of 1832-3, he used phosphorus with remarkable success. It was made into pills according to the following formula:]

Phosphorus, half a scruple; white wax, half a drachm. Incorporate thoroughly in a mortar, a sufficient quantity of water being present during the process to prevent combustion. Divide the mass into ten pills, which are to be preserved for use in a phial of distilled water. In all the slighter cases, in the premonitory diarrhœa, in the more tractable stages of the disease, also as an adjunct to the use of the pills in many cases, the following is the medicine on which I depend:—Strong nitric acid, two and a half to eight minims; tincture of opium, four to eight minims; syrup of saffron, a drachm; water, an ounce and a half: mix for a draught. Except in those confirmed forms where all other means prove powerless, and when I would trust to the pills alone as the basis of treatment, and as the sheet-anchor of the patient's safety, I know of none other of equal utility in cholera.

[In illustration of the mode of employing these remedies, Mr. Batten gives a number of cases, the notes of the first six of which were taken in September, 1833.]

Case 1. Mr. G——, aged 40, residing in Free School-street. I first saw him on the morning of the 24th ult. Had excessive purging, which had been going on for three days, gradually increasing; vomiting had not long supervened; complained of but little pain; evacuations perfectly choleraic—i.e., serous; the sunken countenance, half closed eye, pallor, and coldness of surface, sufficiently indicated that absolute prostration was close at hand; yet as the pulse was not very low, nor the voice much affected, I thought it possible that the case might not be too far advanced to admit of relief by the means usually found effectual in the earlier stages, and therefore prescribed him a draught of the nitric acid and opium mixture. Visited him at two P.M. Worse; vomiting found increasing; cramps in the legs, &c. I now gave him one of the phosphoric pills. Slight improvement soon followed; mitigation of cramps and purging, but not of vomiting; the pill was not ejected. At four P.M. still improving; gave another pill. From this time the patient's recovery was steady and uniform, The cramps first gave way; the purging then ceased; and lastly, the vomiting subsided, and with little further assistance he was quickly well.

Case 2. A female, aged twenty-five, living in Vine-yard. First seen by my brother, about two P.M., 12th instant. Attacked only a few minutes before. Vomiting, relaxed bowels; excessive cramps in all the voluntary muscles; pulse failing; dejections choleraic, and features collapsed. One pill was given immediately; a little cold water occasionally. In ten minutes, another pill was administered; and in ten minutes more, a third pill. The three grains of phos-

phorus thus taken were retained. The cramps rapidly subsided. In half an hour she was free from pain, and the purging and vomiting had nearly ceased; but she was collapsed, listless, cold, and blue, from which state, however, she quickly rallied; and with the aid of a little saline medicine, in two days she was quite recovered.

Case 3. Mr. M——, aged 30. Seized suddenly, while in chapel. Seen immediately; found in a state of complete prostration. Pulse low, (and at intervals imperceptible) voice lost; countenance choleraic; extremities cold; slight vomiting; and there had been some diarrhœa during the morning. A dose of spirit of ammonia and of compound spirit of lavender being at hand, was first administered. He seemed rapidly sinking; was directed, therefore, to be carried into a neighbouring house; and was then treated precisely as the last case. The result was, that in one hour he was enabled to walk home—more than a mile. On recovery, he expressed himself as having suffered severe crampy pains all over, and especially about the stomach and bowels.

Case 4. J. E——, aged 28, a sailor. Seized on Monday, about two p m. This case much resembled the last. The cramps in the legs were more severe; the pulsation was feebler, and soon ceased entirely. He had been considerably purged; had vomited twice or thrice during the previous half hour; and suddenly merged into the state in which I found him—apparently *in articulo mortis*. The treatment pursued was precisely as in the two last cases; in fact, he took no other medicine but the three phosphoric pills, and the result was equally gratifying. In twelve hours he was quite well.

Case 5. Mrs. S——, aged 36, Suffolk place, Snow's Fields, of debilitated and irritable constitution; four months advanced in pregnancy. This was a severe case, exhibiting a concurrence of the most distressing symptoms; constant vomiting and purging; violent cramps; great prostration; the evacuations and countenance truly choleraic. This was on Monday morning.

Treatment.—One of the pills every hour, until three had been taken. The symptoms persisted during the day, although the vital prostration was arrested; in the evening, this was followed by general improvement; the draining from the bowels ceased; the cramps abated; the vomiting subsided; at midnight, although in the blue stage, she was not pulseless; was free from pain, and expressed herself better. Has continued to progress, and is now out of danger.

A day or two after these notes were penned, this poor woman suffered abortion, and sank under the effects.

Case 6. G. J.——, aged 30, a sailor, robust and healthy: was seized yesterday afternoon, just after having eaten a few oysters. Such was the suddenness and violence of the attack, that he was reported to have sunk on the floor as if he had been stunned by an electric shock. At the onset, the cramps were so horrible that they "quite stopped his breath." On his being brought into our surgery, these were renewed with such fearful violence, that the suf-

ferer could neither be kept on his seat, nor prevented from tearing off his clothes; extreme, indeed, must have been his agony. The chief brunt of the spasm appeared to occupy the femoral, abdominal, and thoracic muscles. There had been neither vomiting nor purging.

Treatment.—A draught of strong nitric acid and opium mixture immediately; no amelioration followed. After waiting ten minutes, I gave him one of the pills; the effect was immediate; so rapid was the relief experienced, that in less than a quarter of an hour the poor man walked to his lodgings, a distance of 200 yards. During the remainder of the day, he took a little weak acid and opium mixture, and on this (the following) morning, he called upon me quite recovered.

In addition to these briefly sketched cases taken from my former experience, I shall now add, *more in extenso*, one of recent date, being the only instance in which I have found it necessary to have recourse to the pills during the present epidemic.

Case 7. Mr. H——, aged 68, residing in Ebury street, Pimlico; of nervous temperament, debilitated constitution, and spare habit, with slight hemiplegia from an apoplectic seizure sustained twelve months ago. On Monday, August 6, about 6 a.m , he was seized with diarrhœa. When I saw him, at 9 a.m., he had had a great number of copious evacuations, which, from being at first of bilious character, had now become watery, light, and flaky—almost, in fact, truly choleraic. The pulse was feeble, slight vomiting, some griping pains, and a good deal of cramp in the legs and feet. I prescribed him the nitric acid mixture of the strength of, acid, three minims; tincture of opium, three minims and a half, to each dose. Under the use of this medicine improvement soon set in. At first the draughts were exhibited every hour; but as recovery advanced, the intervals were extended to two, three, and four hours. On Wednesday evening he would, and did, walk out to some distance, which fatigued him considerably. Next morning, about half-past three, he was suddenly seized with diarrhœa. I saw him in about an hour, and found he had had five or six profuse evacuations, and was much exhausted. He had neglected taking his medicine on the previous day, but had taken, since his attack, two small doses that had been left—apparently, without any effect. I now thought I would try the effect of a cretaceous opium draught, to be repeated in an hour. Shortly after he had taken the second draught, I received a message that the patient was dying. On reaching him, at eight o'clock, I found that the purging had been unceasing. The dejections were of the true rice-water or serous character, of which there was now a continuous draining from the bowels; no pain; pulse exceedingly feeble; voice lost, features pinched; diminished temperature; vomiting, thirst, and excessive prostration. I instantly gave him one of the phosphoric pills, and allowed half a wine-glass of cold water as often as desired. Improvement was soon evident. No further vomiting occurred. In a few minutes the draining from the intestines began to diminish;

the pulse improved; he felt "better." The progress of amelioration continuing, at nine o'clock I gave him a dose of the acid mixture, and left him comparatively comfortable.

At ten o'clock I found little further advance had been made, and there had been an occasional oozing from the bowels. Gave him another pill. During the following hour, the improvement was remarkable, and highly gratifying. At eleven o'clock, Dr. Murphy saw the case with me, by desire of some of the patient's friends. At this time all the formidable symptoms had subsided; the countenance, voice, and pulse had much improved; purging quite stopped, and even the thirst had nearly left him, and he only sipped a little rice-water occasionally. A dose of the acid mixture was given, to be repeated every two hours. At two o'clock I found him so much improved that I merely ordered the medicine to be continued.—Evening: still better, and took nourishment.—Friday morning: doing well. At three p.m. this day, Dr. Murphy and myself visited him again: all trace of the disease had left him. To have some beef-tea; continue the medicine every four hours. From this time it is unnecessary to give further details. When I called on Wednesday, I found the old gentleman apparently well, save a little debility, and in his accustomed health, and he has been quite well since.

Lancet, Sept. 22, 1849, *p* 313.

[Dr. C. J. B. Aldis, Lecturer on Medicine at the Hunterian School, confirms the value of the treatment by phosphorus. He says, speaking of this drug,]

The remedy I allude to is phosphorus, which was administered in the dose of one grain, combined with wax, in the form of pill, by Mr. Batten, surgeon, residing in Westbourne-street, Pimlico, with whom I visited the patients. The two persons who recovered resided in Queen-street, Pimlico; one having taken three pills during six hours, while the other took only one pill. The other patient, who lived in Westbourne-street, took two pills; she rallied for a short time, but eventually died, having been in a hopeless state when first seen. Medical men are aware that it is a powerful medicine, and should be used with great caution. Beck says, that two grains and a half, in solution, have destroyed life; and Taylor says, that less than half a grain has produced the same result. Gendrin also considers it to be a dangerous medicine. I would not venture to employ it unless the patient were in state of collapse, and then would it not act more quickly in the form of phosphoric ether—"æther phosphoratus"—dissolved in sugar? I have invariably checked every case of cholera which I have seen, prior to the blue stage, by calomel, combined with small doses of opium, sinapisms, warmth, absorbents, and stimulants. Mr. Batten published his mode of treating cholera by phosphorus in the solid form in 1833.

Medical Times, Sept. 15, 1849, *p.* 225.

Treatment by Nitrate of Silver.

[Mr. GEORGE ROSS found acetate of lead very useful, but, he says, it failed in severe cases. He also tried sulphate of copper, but did not like it. Mr. R. says,]

Nitrate of silver was at last given in this form:—℞. Argent. nit. gr. j.; pulv. opii, gr. $\frac{1}{4}$; Ft. pil. post sing. alvi deject. liquid. sum.

If the purging were very frequent, the pills were given regularly every hour to the extent of five or six doses, or even more if necessary. When the evacuations have been very profuse, two or three pills have been given at a dose, and repeated until the evacuations were partially or wholly arrested,—a result that invariably occurs. This remedy is as certain in its effect of restraining the rice-water purging as opium and chalk-mixture in checking the premonitory diarrhœa.

Since I commenced the use of this remedy, my assistants and myself have treated 853 cases of diarrhœa in all its stages, and of this number, seventy only have been placed under the nitrate of silver treatment, but these seventy were the worst cases, and in the rice-water stage. They were considered by myself and assistants, as they happened casually to come under the care of either, to be beyond the reach of other remedies, and were placed upon the plan recommended as a dernier ressort.

When my assistants find that the purging is not restrainable by the opium, kino, and chalk in full doses, but that the dejections become more watery and copious, the pulse begins to fail, and the powers of the system to yield, they administer the nitrate of silver pills, and I am happy to say, that the effect is so decided that fully developed collapse rarely occurs. The same remedy has been given in profound collapse, and the patients have recovered. The first case in which it was administered was that of a youth of the name of Boak, who was blue and almost pulseless, and profusely purged; nevertheless, the purging was arrested, and the lad recovered. This was the first instance of recovery I had witnessed from a state of collapse so deep.

Seventy cases of the severest kind have been placed upon this treatment from among eight hundred and fifty-three, and, in that number, there have been only five deaths.

I state, again, that these seventy cases were not all cases of collapse, though these and many more of the aggregate number were cases of cholera so called; for I confess myself quite unable to draw the line of distinction, in practice, between cholera and diarrhœa.

They were not cases of collapse, because seeing them early they were not allowed to run into that stage. In the stage of actual and deep collapse, I believe that medical treatment is of little avail, but I am satisfied that the only chance for the patient is suppression of the evacuations.

I may remark, that I have never seen any evil results spring

from its use. It may be employed, with judgment, in the secondary diarrhœa as well as in the primary,—and in any case, indeed, where an arrest of purging may seem necessary; but it must not be given rashly, and when simpler remedies will effect the same end. Vomiting sometimes succeeds an arrest of the purging, but, although troublesome, it does no harm; and, if a little fever should supervene upon the use of the silver, it would, in some cases, be desirable, and assist re-action,—in other cases it can be readily assuaged by the usual saline remedies.

Medical Times, Sept. 15, 1849, *p.* 225.

Remarks by the Board of Health.

[The following remarks are taken from a notification published by the General Board of Health, June 11th:]

Recent experience has fully confirmed the evidence previously adduced, that the localities of this disease and the localities of other epidemics are the same, not a single instance having come to the knowledge of the Board of the spread of this scourge in groups in any other than the ordinary seats of typhus and other zymotic diseases; those seats being uniformly marked by the existence of filth, bad ventilation, overcrowding, and other local causes of atmospheric impurity.

Recent experience further establishes the advantages that have resulted from the operations of cleansing. The first cases of cholera that occur in a locality are sometimes sudden, without any premonitory symptom: this suddenness of attack is in itself evidence of the local presence in unusual intensity of the causes of atmospheric impurity. As soon as cleansing operations have been carried into full effect these sudden cases cease, and instead of them diarrhœa appears, which, if promptly and properly treated, does not pass into cholera. Uniform experience shows that the first and certain effect of these cleansing operations is to stop these sudden attacks; and the cases of premonitory diarrhœa that follow, if early and properly attended to, are, in the great majority of instances, arrested at once; and thus the extension and the great mortality of cholera are checked.

The General Board of Health have to renew their former representation, that what is done against this one disease will have been done against the entire class of epidemic diseases. If, happily, cholera should not again prevail to any great extent, those exertions will be equally effectual against typhus, scarlet fever, diarrhœa, and other native and prevalent epidemic, endemic, and contagious diseases.

Lancet, June 23, 1849, *p.* 675.

[Mr. Ambrose Blacklock, Assistant-Surgeon, Madras Medical Establishment, in a pamphlet on the subject of cholera, makes the following propositions:]

"1. The first alteration in the human body, in the commencement of an attack of Asiatic cholera, is the formation of a pustular eruption in the mucous membrane of the large intestine, and sometimes also in the first six inches of the small intestine, near the ilio-colic valve. These pustules are exceedingly numerous and minute, though mostly visible to the unaided eye, and, like other purulent deposits, are accompanied at first by acid exudations.

"2. The mucous surface of the large intestine is in a positive electrical condition, both before and after the appearance of the acid exudation and pustular punctiform eruption, and during the greater part of the disease.

"3. The mucous surface of the stomach, the skin, and perhaps, also, the bronchial surfaces, are at the same time, in a negative electrical condition, and secrete free alkali, simply because the mucous surface of the colon is positive, and is secreting acid.

"4. The ganglionic system of the abdomen is, therefore, in a state of high excitement, and, from excess of action, resolves the oxygen and hydrogen of the tissues into water, which passes off by the bowel; while the spinal respiratory system is in a state of great depression, and, by its diminished action, permits carbon to accumulate in the blood instead of eliminating it, as in health, from the skin and bronchial surfaces.

"5. The positive excitement of the mucous surface of the colon, and the increased morbid excitability of the ganglionic system, is occasioned by the absence of sulphureted hydrogen from the large intestine, while the depressed state of the spinal respiratory system is occasioned by a want, at the moment, of *free* electricity in the atmosphere to excite the respiratory surfaces.

"6. The pustules pass rapidly through the stage of maturation, and their contents escape into the intestine with the acid fluid, formed by the ganglionic decomposing current: and this mixture of pus and acid serum is the peculiar rice-water evacuation of Asiatic cholera.

"7. The remote occasion of Asiatic cholera is the continual use of a diet deficient in the sulphur required to assist in forming sulphureted hydrogen to preserve the colon, or part of the body requiring sulphureted animal matter, in its normal electrical condition, and so giving rise to morbid ganglionic excitability. By taking a skeleton map of the world, and brushing with colour those parts where the staple articles of diet are deficient in sulphur, omitting only those parts which afford to man abundance of animal and leguminous food, we have in the coloured portions a fair view of the world as it is liable to epidemic cholera.

"8. All the functional and structural changes of the body, occasioned by Asiatic cholera, are immediate or secondary effects of the irritation in the colon. That is to say, for example, the peculiar positive excitement of the intestinal surface increases the energy of the ganglionic centres; the ganglionic nervous currents, thus excited, occasion rapid formation and pouring out of fluid, with congestion and arrest of secretion in secreting organs, and

serious lesions of the spinal cord, all tending to the continued, steady reduction of pulmonary respiration, and to death, either by asphyxia or by the gradual exhaustion of nervous excitability.

"9. Though the presence of this excitement in the large intestine be the immediate cause of the disease, and its arrest by medicine the primary object of all rational treatment, other enlightened modes of treatment must, at the same time, be directed to the other parts of the body, secondarily affected, according to the stage of the disease.

"10. No sanitary precautions of individuals can prevent the occurrence of sporadic cases of cholera, nor can quarantine regulations of governments hinder it from becoming epidemic, unless sulphur, the inorganic conservative principle which prevents the ganglionic over-excitability, be used as a preventive remedy when the individual or the population is scantily supplied with leguminous or animal food."

This pamphlet bears evidence of being the production of one who has been accustomed to see and treat the disease in the country where it originated. The following are amongst its most striking passages:—

"The premonitory stage of Asiatic cholera is characterized by a very peculiar appearance of the person about to be affected. The countenance is said to wear the expression seen immediately before the accession of the cold stage of intermittent fever, but I think it is more like that of a person who has had some vague, undefined, central impression of the bowels being about to move. He seems involuntarily to wish to be quiet, though he has no sick feeling, and does not complain.

"When cholera is known to be at hand, there cannot be an excuse for neglecting to attend to this peculiar warning, and not discerning its significance.

"The powers of life appear, indeed, often to be almost annihilated immediately after this premonitory stage, and sometimes even before the stage has had time to be fairly developed, and deafness and weakness of voice may be remarked even from the first appearance of perturbation in the countenance.

"The odour of the evacuations is like the odour of blood, in the early period of the disease, and afterwards, as the collapse becomes decided, is changed to that of stale meat. These odours from the intestinal evacuations afford, I think, better information of the state of the patient than any other single symptom, and often than any set of symptoms. The voice may be almost gone, the stupidity great, from the deafness and want of vision, the skin very cold, and the jactitation extreme, and still I would have some good hope of the recovery of the patient, if the serous fluid from the bowels have a distinct odour of serum, while I would consider death to be almost inevitable, and that speedily, if the stale-meat odour be once fairly developed."

Mr. Blacklock considers that "death ensues speedily on an attack of cholera, owing to the increased excitability of the ganglionic

system," from the absence of a due amount of sulphur in the system, and he would supply this, and promote a more equal diffusion of nervous energy throughout the system by enemata of

"Hydro-sulphuret of ammonia, one ounce, water, slightly tepid, one quart, thrown slowly into the intestine by the stomach syringe, the patient being held on his right side to forward the passage of the fluid to the colon. This is likely to be of service for a fourfold reason:—First, by the sulphureted hydrogen and ammonia rendering the positive excitement of the intestine immediately negative excitement; secondly, by the ammonia neutralizing the acid of the mucous surface, and converting the pus in the follicles into a gelatinous mucus; thirdly, by directly lowering the ganglionic excitement, as I have already said this gas has a powerful influence in accomplishing; and fourthly, by supporting the general vital power by the diffusible stimulation of the ammonia, and determining the circulation to the surface.

"In cholera," he says, "we have to apply a direct sedative to the ganglionic system, through the colon, and to rouse the spinal respiratory by stimulating inhalations and frictions.

"Tins or bottles of hot water, or heated bricks to the abdomen, indeed, heat applied in an any way to that region, must be extremely injurious in keeping up the ganglionic over-excitement in cholera.

"If you required a galvanic battery to work rapidly, whether would you warm or cool it? Warm it, you will say, through its exciting fluid. Well, then, do not heat the ganglionic system in cholera, for it is too hot already. In fact, the internal temperature of the abdomen during this disease is as high as the healthy temperature of birds."

But amongst sedatives for the ganglionic system, our author decries opium, which he calls "a poison in cholera," and he is cautious how far to advocate bloodletting.

"General bloodletting is contraindicated from the commencement, except in those cases where there is decided fulness or distention of the heart and large arteries, and when, consequently, there is a probability of important organs being injured by having more inspissated blood driven into them than they can afterwards easily free themselves from.

"Before you bleed, try the effects of the hemostatic method of relieving the central circulation—that is to say, place tape ligatures at the insertion of the deltoids, and on the upper third of the thighs, and tighten them just sufficiently to compress the superficial veins. The greater part of the blood, passing into the limbs, will thus be retained in them; they, and not the heart and glandular masses, will be distended; you will have congestion in safe places; and the heart will, in many instances, be sufficiently relieved to render bleeding unnecessary."

Emetics he abjures, adding—

"Vomiting in this disease has long been considered a wholesome effort of nature to relieve the patient of some unknown impurities,

instead of being seen to be, what it really is, in cholera, a symptom of sympathetic irritation in the nerves of the stomach, occasioned by actual irritation in the colon, inducing reflex irritation and reflex muscular contraction, accompanied by alkaline exudation."

Alcoholic drinks he objects to, on account of the depression that follows their use in case their first stimulant effect should fail to rouse the respiratory system; but he advocates the employment of affusion with spirit, which, he affirms, calls the respiratory and cutaneous nerves into reflex action. Epispastics and *calomel* (during the cramps, purging, &c.) he rejects.

"Peristaltic movement of the small intestines is at an end for the time. The duodenum and jejunum remain, throughout the disease, just as they were at its commencement, as is often proved by their contents after death; and the former bowel cannot, therefore, propel calomel, nor bile either, even if the stomach could retain that medicine unchanged, and pass it on."

Sponging the surface with a weak solution of nitro-muriatic acid, "to change the whole surface, if possible, from negative to positive electric;" giving drinks of water, similarly acidulated, and *after vomiting*, with the view of neutralizing free alkali, secreted by the stomach, &c.; calomel after the cramps and purging, in full doses and then with opium; and sulphur, both during convalescence and as means of prevention, are the main remedies recommended by Mr. Blacklock, who remarks—

"I hope yet to see the day when sulphur in small quantities will be regularly issued to every soldier in the field, in India, say forty grains per day, while actually marching, and twenty grains per day, during halts, as a sure way of warding off this terrible disease; and I have a firm belief that sulphur so employed, will be as effectual in banishing cholera from our armies, as lime juice has been in eradicating scurvy from our fleets."

On the subject of diet for Europeans, in hot climates, he says—

"If a native of India eat a large meal of rice, it is speedily exhaled in the form of carbon, from the lungs, liver, and skin, but chiefly from the dark, decarbonizing skin; but if a European endeavour to subsist on rice, he can only get rid of the superabundant carbon by the liver and lungs, as his skin is not a decarbonizing organ to the full extent it is in the dark races, and the liver and lungs must consequently have too much work thrown upon them.

"Rice is not, in itself, deleterious, except in so far as it may burden the respiratory organs and lead to congestion; and it has no power in itself of inducing cholera in Europeans, as it was once supposed to have, although it undoubtedly often does so, indirectly, by being taken in quantity, to the exclusion of a pea and animal diet, the only food which can impart real healthy strength in this country, and give a man a fair chance of escaping cholera by keeping in check the excitability of his ganglionic system. When cholera is likely to become epidemic, soldiers ought to be well supplied with pea-soup, made with gram, or, better still, with minimuloo, in the absence of the dhal or pea of this country, and ought to be encouraged to take

it in preference to rice or bread. They would thus have better legs for the road, and less liability to choleroid disease."

Lancet, July 7, 1849, *p.* 14.

[An anonymous writer in the LANCET recommends quinine with calomel and opium. He says:]

I cannot forbear recording my testimony to a combination of medicines, which, in warding off and checking an attack of cholera in numerous instances, both in Europeans and natives, has been followed with the happiest results. The remedy consists of quinine, calomel, and opium, in the quantities of six, four, and two grains respectively, followed by a wineglass of brandy diluted with a little warm water. This given within the first hour or two of seizure, will be found, in a large proportion of cases, to check the disease.

Lancet, Sep. 1, 1849, *p.* 237.

[Dr. P. NIDDRIE's experience has convinced him,]

That cholera, even in the stage of collapse, is quite as manageable as any other severe disease. The mode I have followed differs little from what I find Dr. Ayre so zealously recommends, and consists in placing on the tongue two grains of calomel, mixed with a little sugar, to be washed down by an effervescing draught. If rejected, the dose is repeated immediately; if not followed by vomiting, the dose is repeated in from five to ten minutes, till reaction commences, when the period between each dose is gradually extended to an hour or longer. Calomel and carbonic acid gas seem to be as powerless as other means, unless thrown constantly into the stomach; therefore this mode of giving them must be followed, and the course of the disease being so very rapid, the patient must not be left until collapse yields to the constant attacks of these remedies. I have had ample experience of the total uselessness, in cholera, of large doses of medicines given at long intervals, and am fully convinced of the great value of calomel and carbonic acid gas, constantly and perseveringly administered at short intervals. Under this treatment purging ceases, or becomes less frequent, although vomiting may occasionally recur for some time; the expression of the countenance gradually changes; the temperature of the breath and tongue rises; the pulse gets stronger and the skin warmer; cramp becomes slight, and extreme collapse is followed by little or no febrile excitement; and the gums do not even swell if care be taken gradually to discontinue the calomel by extending the period between each dose as collapse disappears.

Lancet, Sep. 1, 1849, *p.* 235.

On the Injection of Morphia and Calomel into the Veins in Cholera.—By Dr. R. M'GREGOR, Physician to the Glasgow Infirmary.

[Observing the little power of absorption in the gastro-intestinal canal in the stage of collapse, Dr. M'Gregor was led to employ injections into the veins. He says:]

A solution was prepared by dissolving five grains of the crystallized muriate of morphia in an ounce of pure hot water. Thirty drops of this solution, mixed with six ounces of pure water, at a blood-heat temperature, were injected into one of the veins of the arm by means of the following apparatus, which was adapted by Mr. Hilliard, cutler to the Infirmary, and consists of a powerful six-ounce brass syringe, with a stop-cock on the point, and of a silver tube, being the inner eyed half of a No. 2, male catheter, (from which the outer half had been cut off), and having attached to its cut end a brass-mounting to screw upon the point of the stop-cock or syringe. At first the ordinary lancet was used, but at times there was a difficulty in introducing the point of the catheter or silver tube. I then had prepared a modification of Syme's abscess lancet, by cutting off a portion of its sharp point, and removing its curve anteriorly, retaining the handle. A probe was also used. The operation now consisted in filling the syringe as above, in expelling a small quantity of the liquid so as to ensure the absence of air, then turning the stop-cock, and laying the instrument aside, until the vein was opened, which was done as follows:—the skin over the vein, bandaged above, as usual, was pinched between the finger and thumb transversely, and transfixed with the lancet so as to expose the blue contour of the vein, which was now entered by the lancet, introduced in the usual manner, and retained; the handle is turned to the horizontal, so that the blade opens up the incision laterally. The probe is now introduced behind the blade, which is then withdrawn; while the former is left, and can be retained within the canal of the vein when the patient is being bled, if you desire it,—or if not, until you leisurely introduce behind its flat end the point of the catheter of the syringe, Satisfied that it is fairly within the tube of the vein, you remove the ligature from off the arm, turn the stop-cock, and then cause the liquid to glide easily in by pressing the handle of the piston rod till within half an inch of home. It is desirable to proceed no further, in case of any air having being admitted by the side of the rod. There is a risk of inducing inflammation if you miss the vein and inject into the cellular membrane. There is also an immediate feeling of pain, whereas in injecting into the vein there is none.

The effect of injecting the solution of morphia was highly encouraging; it checked the vomiting, purging, and cramps in most cases, and the patients declared themselves relieved; some slept, and others said they felt as if becoming drunk. The operation as above detailed, is simple, and its mode is readily acquired,—so much so, that the indefatigable cholera clerks, Messrs. Willis and Jacob, at last injected the prescribed quantities in their respective wards, the one without the aid of the other, but simply with that of the nurse.

For injecting large quantities of liquids into the veins, the apparatus employed by Dr. Robertson of Edinburgh, would have been preferable; but our object was to inject a quantity not exceeding six ounces, and for this I would decidedly prefer the syringe. Calomel can

be administered by the mouth when the discharges have been so far checked by the morphia; or mercury may be introduced into the veins in one of the following forms: Take one grain of corrosive sublimate; five grains of iodide of potassium; warm water an ounce. Dissolve. A liquid drachm of this solution, mixed with six ounces of warm water, may be injected as above, and repeated if required. Corrosive sublimate coagulates albumen, although an excess of serum, containing the albuminate of soda, redissolves the coagulum; and hence the reason why the above solution, which has no tendency to coagulate the serum of the blood, was employed. Or.—Take of the cyanide of mercury, in crystal, one grain; warm water, an ounce. Dissolve. A liquid-drachm of this solution, containing one-eighth of a grain, may be used as above. These excite the action of the liver, and produce copious biliary stools. This I have verified upon many dogs, as well as upon cholera patients. A middle-sized dog (of thirty pounds weight) can bear a quarter of a grain, and recover perfectly; but half a grain has killed him. It is well known, however, that dogs will not bear mineral purgatives so well as vegetable. In some cases of extreme collapse we observe dark ecchymosed looking patches, more especially at the inner canthi of the eyes,—seemingly from a stagnation of black blood. Now, it is remarkable how readily after the establishment of reaction these are removed. It is also singular to observe how much the blood alters in appearance and specific gravity; for in individuals labouring under the coma, who have had two or three days convalescence between the collapse and coma, and who during that period imbibed a considerable quantity of albuminous or milky fluids, the specific gravity of the blood stood only 1.050, and it was invariably of a scarlet red.

Medical Gazette, July 27, 1849, *p.* 142.

Russian Treatment by "Samboul."

[Dr. GRANVILLE gives the following account of the most improved mode of treatment adopted in Russia, and under the use of which, he states, nearly 400 patients out of 617 have recovered. He says]

The method here adopted with a patient just admitted into a hospital in a state of collapse from cholera, is to strip him instantly, and lay him, stretched at full length, on the webbed bottom of a couch raised about one foot from the ground. A series of semicircular ribs, like those in our large waggons, moveable towards both ends, and bearing a coverlet of coarse flannel, or a blanket, are then drawn over him, inclosing him thus under a covered ceiling. A couple of nurses bring each in their hands, by means of an appropriate handle, a square iron pan, holding a red-hot square pig-iron, two inches thick, over which, at the very moment of introducing the said pans under the couch, a jug of water is thrown, and the vallance of the couch is let fall around to the ground. The immense volumnes of steam arising from the apparatus fill presently, and are retained within the covering, raising

in a quick time the temperature of the patient to a very considerable heat, followed by perspiration; upon which he is transferred to a warm bed, and there attended to in the usual manner through the several phases of the disease, which it is not the purport of this letter to detail, as every medical practitioner in England is supposed to be familiar with it. I need scarcely add that friction is also occasionally employed.

But there is a point in the Russian practice equally novel, I think, which I desire to mention, and that is the recent substitution of a vegetable substance, first recommended by Dr. Tittman, chief physician to the Petropawlosky Hospital, in lieu of the ammoniated, as well as for all spirituous stimulants employed, with the view of dispersing the state of stagnation of blood and inward inanition. This substance was made known to me under the name of '*Samboul*,' said to be the produce of India, and to have in every case led to the most surprising results. It may be given in substance, but it is more generally employed in the form of decoction or infusion, by spoonfuls frequently administered. Its power, however, in the way of reanimating the dying energies of a collapsed patient has been such as to demand great caution, lest it should produce cerebral excitement, and thence a typhoid fever—a consequence that has often been witnessed in the Russian hospitals subsequently to the recovery from the first or more dangerous state of cholera.

Medical Gazette, Aug 17, 1849, *p.* 286.

Treatment by Wet Sheet and Chloroform.

[It appears that the wet sheet has had a fair trial upon cholera cases at the London hospital, and that though it had the effect of producing reaction, all the patients died. The same fatal result followed the exhibition of chloroform, both by the lungs and stomach. As the editor of the Medical Gazette observes,]

Facts of this kind properly recorded will clear the way for improved treatment. If we cannot yet suggest any successful method of treatment, it is something to know what to avoid, by keeping an honest record of those plans which have been fairly tried, and have been found to fail.

Medical Gazette, Aug. 17, 1849, *p.* 298.

Treatment by Matico.

[Mr. T. Slipper, of Kennington, says]

I have at length been induced to treat it *as a simple* hemorrhage, from the serous vomiting and purging leaving the blood in such an inspissated state as to render it incapable of circulation, and thus inducing rapid collapse, as in uterine and other hemorrhages. This practice has almost invariably proved successful. I find unless the styptic be very strong, it has only a partial effect; but, if given in the subjoined form, it arrests the vomiting for a time,

and very frequently produces a permanent effect; it is as follows:— Take of tinct. of matico as strong as it can be prepared, from ʒij to ℥ss.; tinct. of opium, ʒss. to ʒj.; sp. of camphor, ♏xv. to ♏xx.; water, ℥ss. M. A fourth part to be taken whenever the sickness or purging comes on: two doses generally suffice for some hours. Should there be any return, a repetition of the medicine again arrests it, and by an occasional recourse to it for two or three days, my patients have in every case completely recovered from the disease.

Medical Gazette, Aug. 17, 1849, *p.* 296.

Spanish Treatment by Olive Oil.

[The following treatment is highly lauded by a Dr. Pedro Vazquez, who professes to have used it with great success in Spain, during the epidemic of 1833. It has, however, signally failed, when tried recently at St. Bartholomew's and St. Thomas's hospitals.]

Immediately on being attacked the patient is to take three small cups of pure olive oil, allowing eight or ten minutes between each dose. A quarter of an hour after taking the last (or sooner if vomiting has commenced) abundance of water, rather more than lukewarm, is to be drunk until the person vomits, which result should be excited by the introduction of a feather, saturated with oil, into the throat. A slight rest can then be allowed, when more warm water should be administered (though no more oil); but when the vomiting fatigues too much, a stop should be put to it by giving a large glass of cold water..... It has been observed that many cannot take the three cups of oil, as indicated, because the first causes sickness, but this will be obviated by giving a little cold water after each dose, which must all be taken, as the grand object is to varnish the intestines with oil..... The oil never does harm, but, on the contrary, from the moment it enters the stomach it destroys the force of the poison which produces the cholera. If the invalid on the following day exhibits a foul tongue, a very small cup of the oil may be given, with a little cold water after, and, at the end of one hour, sufficient warm water to cause vomiting as before.

All medicines and other treatments are prohibited by Dr. Vazquez, who states that the cholera can thus be cured in a few hours, and with less trouble than a cold or rheumatism.

Medical Gazette, Aug 17, 1849, *p* 283.

Treatment by Creosote.

[Mr. C. N. Spinks, of Warrington, has used creosote extensively in the treatment of diarrhœa, and has also given it in cholera. Out of a large number of cases of the former disease, he tells us]

The first 93 cases of diarrhœa were treated with the usual chalk mixture and opium, the remainder with creosote: in those treated with the former the diarrhœa continued for some days, and, when checked, was invariably followed by a disagreeable rumbling and flatus in the bowels. In the 131 cases treated with creosote, the diarrhœa *immediately* ceased, and was followed by none of the above symptoms. In the twelve cases of rice-water purging, the effect was instantaneous, the first dose generally putting a stop to the discharge. In the eighteen cases of cholera, all of whom had vomiting, rice-water purging, cramps, and blue skin, creosote had the same decided effect, at once checking the purging and vomiting, the cramps very soon afterwards subsiding, the pulse becoming full and soft, a free perspiration breaking out over the body and extremities.

Of the eighteen cases of cholera treated with creosote, I have only lost *two*, these being far gone in collapse when I was called to them. The formula in which I use the creosote is—

℞. Creosote, ♏xxiv.; mist. acaciæ, ℥ss.; sp. ammon. c. camphora, aa. ʒij.; ether. chlorici, ʒij.; aquæ, ℥viss. M. ft. misturæ. Capiat cochl. ij. mag. omni hora.

In simple diarrhœa I only give two drops every two hours, with the above stimulants, and no astringent whatever

Medical Gazette, Aug. 10, 1849, *p.* 254.

Treatment by Galvanism, &c.

[At a Meeting of the South London Medical Society, Dr. P. Murphy remarked, that]

When the collapsed stage arrived, unless galvanism did something, no other remedy was capable of propelling the crassamentum of the blood through the veins, separated as it was from the serum, which had almost disappeared. Nothing but a solid kind of coagulum remained in the veins and arteries; and how the heart was to be made to push this coagulum through was a problem he never had been able to solve.

Medical Gazette, Aug. 24, 1849, *p.* 332.

[Mr. T Weeden Cooke gives the following account:]

One of the Tooting children was seized with cholera in the Royal Free Hospital. The patient was a girl about eight years old, under Dr. Peacock's charge. In about four hours she became completely collapsed, the power of deglutition had ceased, and consequently all internal remedies were useless. Dr. Peacock applied one pole of the galvanic machine over the heart, the other over the region of the stomach, or rather of the solar plexus. In half a minute the child began to rally, some strong beef tea was got into her stomach in less than ten minutes, and ultimately the resurrection was complete.

Mr. Blacklock asserts, in his pamphlet (p. 37,) that

"The sure method of restoring vital heat is to excite the respiratory muscles by sending an electro-magnetic current from the spinal respiratory bodies to the diaphragm. To accomplish this, have each wire of the machine furnished with a thin, circular, very soft metal plate; one plate two inches square for the positive wire, the other at least six inches square for the negative wire, and attached to the end of the wire by a small eye, like the eye on the reverse of a button. Bandage the positive plate to the upper part of the nape of the neck, and the negative plate to the upper part of the epigastrium; and then imitate the natural respiratory current by having the battery well charged, and the break of the machine so arranged that the succession of shocks will occur at the most about twenty-five times in a minute."

He adds, in a marginal note, that

"Oxygen has often been recommended as a remedy in cholera; but it is not nearly so much required as stimulation by electric impulsion. There is enough of oxygen in the air at the worst cholera periods, but there is no free electricity then to cause it to be combined intimately with carbon."

Lancet, July 28, 1849, *p.* 95.

On the Internal Employment of Severe or Benumbing Cold in Cholera.—By Dr. Jas. Arnott, Brighton.

I have to propose, as a remedy in cholera, the application of a degree of cold adequate to suppress the irritation of the digestive mucous membrane; and the unequivocal success from this practice in two cases, if it does not fully prove its value, strengthens, at least, the theoretical foundation of the recommendation.

The cold, to be effectual, should be of an intensity capable of quickly benumbing a finger immersed in it, or of about ten, or at least six degrees of Fahrenheit's thermometer below the temperature of dissolving ice. I think this will be an adequate degree of cold, provided the draughts communicating it be sufficient in quantity and often enough repeated: it proved to be so in the cases referred to. The draughts exhibited in these were prepared by putting a cylindrical tin vessel or canister containing about eight ounces of water, and having one or two teaspoonfuls of sugar or salt dissolved in it, into another larger vessel containing the usual freezing mixture of ice and salt employed by confectioners. The solution in the canister had been previously cooled to 32° of Fahren., by dropping a little pounded ice into it; and when it began to freeze, it was constantly stirred until the whole was converted into a thick gelatinous substance. To this jelly, a teaspoonful or two of salt was added just before it was administered, for the purpose of still further reducing its temperature; and from six to eight ounces, or a tumblerful, was taken as a draught. A little lemon-juice will remove much of the nauseous taste of the salt.

It is almost unnecessary to observe, that until the particles of ice

in the draughts are all dissolved, the intense cold will be preserved; whereas, when merely iced water is swallowed, its temperature begins to rise even before it has reached the stomach.

The cases of cholera in which this expedient has been tried, were those of a boy, aged fourteen, and of a man upwards of seventy, patients of the dispensary. To both, opium and calomel had been exhibited before the trial of cold, but without arresting the discharges, or being otherwise of apparent use. The discharges in the case of the boy were of the rice-water character, and collapse was imminent; in that of the old man, the copious excretions were still, in some degree, bilious; he was tortured with cramp, but his pulse was still very perceptible. Both patients took about two pints of the jelly-ice above described; and about a pint of the same mixture was in the boy's case administered by injection. The beneficial effect was soon discernible in both instances, but more quickly in that of the adult, who took the draughts more readily and in quicker succession. In half an hour the vomiting had entirely ceased, and the cure was completed by calomel and opium. In the other case there was no vomiting two hours after the first exhibition of cold draughts; and upon the cessation of the vomiting the pulse acquired strength, and the countenance animation. Both patients soon threw up the draughts containing most salt, but not until the cold had produced its full influence; and the stomach being thus emptied, the succeeding draught had, of course, more power. There are other modes of producing intense cold noticed by chemists, and applicable to such a purpose, which might perhaps in some cases be preferable.

The possibility of thus administering ice as a jelly is important as respects its use in other diseases accompanied with irritation of the stomach. Too often has the administration of ice in disease been a mere delusion from the difficulty of swallowing it quickly enough to produce any useful effect.

Medical Gazette, Aug. 31, 1849, *p.* 374.

Treatment by Iced Water.

[An anonymous writer believes cholera to be]

Hemorrhage, i. e., that the serum of the blood is separated from the crassamentum, and the capillary vessels pour out the former from the mucous membrane of the stomach and bowels, and that those of the skin also lose their power. The larger quantity that is discharged from the former is owing, in my opinion, to the heat being so much greater than on the surface, and that that which is called cold sweat is not sweat at all, but an exudation of serum. Taking this view of the case, such remedies should be employed as are calculated to check ordinary hæmorrhage, such as iced water freely administered internally, as well as sponging the whole surface of the body, in the first place, with cold water, and then wrapping the patient in a wet sheet (cold), and covering him with blankets. The effect of this will be to check all further exu-

dation, by which you will generate heat, and natural sweating will be restored, as well as the secretion of the liver, kidneys, &c. The usual styptics may be employed, in addition to the above treatment, if thought advisable by the medical attendant.

You will perceive by this that I object to the use of brandy, opium, warm applications, &c.

P.S. I think the exhibition of olive oil well worth trying.

Medical Gazette, Aug. 17, 1849, *p.* 286.

Treatment by Cold Affusion in Berlin.

[Mr. J. Lamprey gives the following account of the manner in which cholera was treated in the hospital "La Charité," at Berlin.]

"The remedies mostly used in the treatment of this disease were, the cold affusion, and cold fomentations to the abdomen and head; frictions of warm flannels, or sometimes jars of hot water, were applied to the feet, to keep up the temperature of the extremities. The medicinal agents, with few exceptions, were not selected from any particular class, or for any other purpose than to meet symptoms.

"The patients, when admitted, were almost too far gone for any remedy to be of use.....They were always received in a state of syncope or asphyxia, and required some powerful stimulant to restore the suspended animation; for this purpose the cold affusion was found the most powerful and expeditious agent.

"The manner of applying it in the hospital of Berlin was as follows:—As soon as the patient was stripped of his clothes, he was placed in the wooden bath before mentioned; one assistant-nurse supported him in an upright sitting position, whilst another assistant dashed a bucket of cold water on his head and back; he then took another bucket of cold water, and poured it equally quick on his face, thorax, and abdomen. The patient was then partially wiped dry, and lifted into bed, when sometimes he was wiped drier, and hot jars applied to his feet. As soon as this was done, a large sheet, saturated in cold water, and folded up, was placed on his abdomen as a cold fomentation; another smaller piece of wet linen was placed on his head. This process was repeated three times in the twenty-four hours.

"I had an opportunity of witnessing this process and its effects on several occasions. On examining a patient in a short time afterwards, I was surprised to see the immediate restoration of animation take place, though the patient had been pulseless and cold before he was placed in the bath, yet in a few moments the pulse could be felt, and an increase of warmth perceived, which gradually improved. In some cases the patient began to perspire profusely, and recover immediately after the first or second application.

"The medicinal agents used were derived from no particular class, one excepted, which was used as a specific only, by the Persians originally, and lately by the Russian physicians. I allude to the radix sambul. It is a root of a very peculiar volatile odour, strongly resembling the perfume of violets, on which, in all probability its

virtues depend, as great caution is required in its preparation, in infusion and decoction.

"The issue of the case in which this has been tried should certainly be no criterion of its value. An emetic of a scruple of ipecacuanha and one grain of potassio-tartrate of antimony had been administered in one case, probably on the same principle that it is sometimes given in fevers—namely to cut short the attack. Unfortunately, where this had been tried, though the case appeared favourable enough for it, it did not succeed; the patient died in a few days after, from some cerebral complication.

"Dr. Ward, of the Bethanien, informed me that he treated two cases with nux vomica, giving ten drops of the tincture three times a day; and that one case recovered, the other sunk. He also told me that a friend of his gave charcoal with good success.

"Chloroform had been tried on two occasions; the first case derived a temporary benefit from it, as it completely subdued the spasms, and afforded tranquility for about three hours; but its effects were not permanent, as these symptoms were renewed as soon as its effects were over. From the partial success in one case it was tried in another; but unfortunately *immediate death was the consequence.*"

The success of the "cold affusion" plan even Mr. Lamprey allows not to have been very encouraging,—"the cures being only in the ratio of sixteen per cent. So much for the *cold-water* system.

Lancet, July 7, 1849, *p.* 16.

Treatment by Emetics, &c.

[At a meeting of the South London Medical Society,]

Mr. Greenwood stated that he had prepared an official report with respect to the state of the public health in Clapham. The cholera was confined to peculiar localities. In the Wandsworth road as many as thirty deaths occurred in seventeen or eighteen houses; and a very large mortality occurred in another but smaller district. Having traced the existence of the cases in the Wandsworth road to imperfect drainage, bad water, and the condition of the neighbouring sewers, it was of importance to consider how far the same causes had operated in other localities. In 1832 the cholera seemed to follow the same course, crossing Battersea fields, and passing up Acre lane, to terminate in the same spot where it had now proved so fatal. In Park lane, Acre lane, there were last month, in about ten houses, no fewer than sixty cases of cholera with collapse, of which eight proved fatal. In almost every case there were premonitory symptoms of diarrhœa. In Wandsworth most of the cases occurred without any premonitory symptoms whatsoever. The first case to which he should refer was that of a young man, twenty-one years of age, residing in Cook's buildings, Acre lane. He had been to Bucklersbury, in the city, with his master. He was there seized with vomiting, cramp, and diarrhœa. He returned to Clapham. Within ten minutes after his return he

was in a state of collapse. Adopting the plan of treatment used by Mr. Pugh, of Gracechurch street, he (Mr. Greenwood) administered an emetic in the first instance, namely twenty-five grains of ipecacuanha. Under the particular mode of treatment which was so commenced, nine out of the ten patients were found to recover. He had adopted it with success. The man recovered. The case was seen by Dr. Silvester, Mr. Taylor, and himself. The next case was one which was subjected to the same mode of treatment; the man recovered in twelve hours. The third case was a fatal one; it had advanced too far; he was told of it at one, and the man died at four o'clock. There were in the same place, two other cases. The man's child was seized with the disease and died; and his wife, who was also seized, died in about eight hours. In each of the houses immediately adjoining, cholera broke out, and one person died in one of these houses two days ago. About twelve cases occurred in the locality where the first case of fatal cholera was observed. In other cases recovery was effected by the treatm nt to which he had referred, the emetic being always followed by a dose of ether, opium, and chloroform. The effect of the emetic seemed to be that the secretion became tinged with bile, and where that was the case he had found invariably that recovery followed.

Dr. Lloyd observed that they were all agreed in thinking there was some influence in the air which produced certain kinds of complaints at particular periods. The same cause might give rise to other complaints at particular periods. It was their duty, then, to discover what were the causes which gave rise to this disease. He then described two localities which had come under his observation; the first being Silver street, Rotherhithe. The privies entered into a drain which had once had communication with the Thames; but that had been cut off. All the privies then ran into the cess-pool. There were eighty cases and thirty-eight deaths in the course of a fortnight. The water of the well from which the inhabitants derived their supplies, smelt of hydro-sulphuret of ammonia. No sooner had the communication of the sewer with the Thames been opened, the sewer cleaned out, and the people prevented from drinking the water of the well, which was filled up, than cholera ceased. Within half a mile of the same place stood two rows of houses back to back. The privies of seven houses on the one side emptied into a sewer, which opened to the Thames at one end. In those houses there had been no fewer than twenty-five cases of cholera, and fourteen deaths. One house had a pump-well railed off, to which the other houses had no access. Only one case had occurred there. But the inhabitants of the other houses obtained their supplies of water by going to the sewer. That was the water they drank. In Charlotte row they went to that sewer to get water. In the last nine days there had been no fewer than eight fatal cases of cholera. If anything were required to prove the effect of local causes in producing cholera, these two instances are sufficiently strong. When cholera was raging in Silver street. it was not so in any other part of Rotherhithe. There were other cases which proved the disease

to be contagious. In Gillham's court a man, his wife, his son, and his daughter, were seized within twenty-four hours, and died. The nurse did not complain at the time; but some persons having gone into the house found her there lying dead. Near the house was a privy, which, owing to a quarrel, had not been cleaned, and which was creating a great pestilence. Six persons died in that house, but none in the next, which stood in the same position as regards the privy. If the local cause only were sufficient to account for the cases that had occurred, it ought to have produced the disease in both houses. Some of these six cases ought rather to be attributed to contagion. In another case a woman bought a bed which had belonged to a man named Haynes, who had died of cholera in Lower Rotherhithe. She and her children, living about 300 yards from his house, slept on the bed, were seized with cholera, and died. They did not know that it had been in possession of a man who had died of cholera. In their own immediate neighbourhood there was no cholera. There was but one case in the same street. The nurse who attended Haynes died. Mrs. Reid, a remarkably healthy woman, who nursed a man named Moore, was seized, and died. Two cases occurred on board the Betty, in the Surrey canal. The captain's wife, who lived at Holloway or Hampstead, came to see her husband, and finding two men ill, waited on them; next day she was seized with cholera and died. Mrs. Snowden came to nurse Captain Bews, and left without symptoms for a place where there was no cholera. She died; her child also died. Her mother came from Bermondsey to nurse her. The mother was not infected in Rotherhithe, but went home, was seized, and died. A tinman, in Bishopgate street, had gone to Shoreditch to see a cousin, who had cholera. Three days after, at his own residence, he was seized with cholera, and died in twelve hours. Four hours after his wife was seized, and died in about twenty hours. His sister, who attended him, was also attacked, and died. The case occurred at the same time Mr. Key was so ill. The man had sent his clothes to a woman who lived at the further end of Bishopsgate to be washed. After she took them home she died of cholera. The conclusion to which he had arrived was that cholera, like typhus, was, in certain cases, contagious. In Guy's Hospital, with which he was connected, typhus was not so, because, by judicious distribution, that hospital had never more than three or four cases in one ward. But no one could doubt that at a fever hospital, or as in Edinburgh, where the infirmary had distinct wards for fever, the surgeons, nurses, and others, became liable to fever. If the poison of typhus could be so intensely concentrated as to make it contagious, might not cholera also be rendered intensely so? This, at least, the profession had learned from what had taken place—they had learned a good many modes of treatment which were to be avoided, and they had also learned some which had proved successful.

Medical Gazette, Sept. 7, 1849, *p.* 429.

Treatment by Chloroform.

[Dr. JOHN DAVIES, of Hertford, gives chloroform internally, both in the premonitory diarrhœa, and in the stage of collapse. He says,]

It should be given mixed with mucilage, in doses of from ♏v. to ♏xv. every hour or two. One dose will frequently stop the diarrhœa and sickness. The patient should drink plentifully of milk and cold water, with a little sesquicarbonate of soda in it. The warm bath, with a large quantity of common salt in it, frequently repeated, is a valuable auxiliary. But chloroform is the remedy.

Medical Gazette, Sep. 14, 1849, *p.* 379.

Treatment of the Premonitory Diarrhœa by Pernitrate of Iron.

[Dr. T. REYNOLDS, of Brockville, says,]

I have for some months past directed my attention to various preparations of iron, the tr. mur. ferri among the number, to check the diarrhœa which precedes the common cholera of the country, and have found a preparation of the pernitrate preferable to all others. In no instance have I found it to fail to check the first onset of diarrhœa; and in many severe cases where the disease had made considerable progress, I have easily controlled its advance by the administration of a few doses of the preparation. It is frequently necessary to remove some obstruction of deficient secretion, by the use of a dose of calomel or castor oil; of course the practitioner must be guided by the circumstances of the particular case.

Mr. Kerr, of Dublin, some years ago, recommended a preparation of the pernitrate, made by acting upon iron by dilute nitric acid, till a saturated solution was obtained, then adding a small quantity of hydrochloric acid to prevent decomposition. Notwithstanding this addition, a deposit generally takes place of the sesquioxide, which of course is an objection to his preparation.

Mr. Duhamil found that the magnetic oxide was changed into the state of sesquioxide (see Am. Journ. of Pharm., vol. xvii. 1), and he recommended the addition of a quantity of sugar instead of the hydrochloric acid, to prevent this contingency. His formula appears to be a valuable one, and is as follows:—Take of iron wire free from rust, and cut in several pieces, 6 drachms; nitric acid, 1½ fluid ounces; water, 8 fluid ounces; sugar, 14 ounces; dissolve the iron to saturation in the dilute acid, by allowing them to stand 12 hours, with occasional agitation; filter and add the sugar which is dissolved by a gentle heat, and the solution filtered if necessary. One reason for preferring this preparation is, that the iron is here in the state of magnetic oxide, and it has been everywhere observed during the prevalence of cholera, that the atmosphere is singularly deficient in electricity; the employment of agents likely to supply electricity, or its closely allied agent, magnetism, is worthy

of attention(!) To each ounce of the above syrup I add of tinct. opii. ʒi., sp. camphor. gtt. xxx.; or where there is much pain, of the tinct. capsic. ʒi. to the ounce of syrup. The dose of the mixture, 16 to 20 drops in a little water, repeated every hour, till relief is obtained; should there be much pain or prostration, the dose to be administered in a little burnt brandy.

I should be glad to hear that this preparation has been made a successful trial of, as it has in my hands proved a valuable auxiliary in the treatment of a very troublesome and often alarming class of complaints with which we are every summer more or less visited. —*British American Journal*, August, 1849.

Medical Gazette, Sep. 21, 1849, *p.* 521.

Treatment by Chlorine.

[Mr. C. W. Mann, of Clerkenwell, recommends the liq. chlorinii of the Edinburgh Pharmacopœia, a concentrated watery solution of chlorine, with a little sulphate of soda. He says:]

Within the last four days I have used it in nearly one hundred cases—varying from the mild form of diarrhœa to severe forms of cholera. It has invariably stopped all pain. purging, and vomiting; I have only lost two cases, both of which were far advanced in the stage of collapse when I was first called in. The dose I give is from ʒj. to ʒiss. diluted with water, every hour until the symptoms are relieved.

Medical Gazette, Sep. 21, 1849, *p.* 506.

Treatment by Injections into the Bladder.

[M. Piorry recommends injections into the bladder.]

Among the remedial measures proposed, we must not omit M. Piorry's. This gentleman, considering that cholera patients die for want of water in the blood, had this liquid thrown, in large quantities, into the *bladder* of a man dying of the disease. No less than two quarts were injected in one hour, and absorption was rapidly proceeding, as ascertained by the plessimeter; the pulse rose; the veins, which were almost empty, began to refill, and the man was improving.

Lancet, June 30, 1849, *p.* 699.

181.—ON THE PATHOLOGY AND CAUSES OF CHOLERA.

I. Pathological Appearances.

[The *post-mortem* appearances after death from cholera have been examined on a large scale and recorded in minute detail during the late epidemic. MM. Thologan and Levy, at the Hôpital du Val-de-Grace at Paris, and Dr. W. T. Gairdner, at the Royal Infirmary of Edinburgh, have, in particular, given elaborate accounts of the appearances presented by large numbers of cholera subjects. These accounts will be found in the London Journal for August, 1849, p. 750, and the same Journal for October, 1849, p. 929. It is to be regretted that the careful and laborious investigations of these gentlemen should have been so unsuccessful in elucidating the true pathology of the disease. The observations of the French pathologists have been carried to an extreme, and even ridiculous degree of minuteness: thus, they speak of "rose-coloured patches" as distinguished from "red patches," of "thick arborizations," and "fine arborizations." We therefore prefer to follow Dr. Gairdner, and would thus epitomize the results of his investigations, *which are, in the main, of a negative character.*

1. The subjects attacked, seemed to be chiefly persons in health, or those free, at least, from acute or actively progressing disease. It appears, says Dr. Gairdner,]

That the opinion which has been so extensively prevalent since 1832, that cholera attacks chiefly or exclusively individuals of unsound constitution, or bearing the traces of previous organic disease, is not borne out by the facts of morbid anatomy; and, in particular, that there is no evidence whatever that previous disease of the intestinal canal predisposes to cholera.

[2. The nervous system presented no lesion worthy of remark.

3. The blood did not differ in colour or coagulability, from its usual condition. The serum was remarkably viscid, chiefly in cases fatal during collapse or early reaction; this viscidity being due to the loss of fluid matter by the intestines. *Congestion was not at all an essential or universal appearance.*

4. The liver appeared healthy, though mostly paler than usual; and the gall-bladder was usually full of healthy bile.

5. The kidneys were in many cases in a similar state to that which occurs after scarlatina, "the cortical substance being pale and turgid, and the tubuli uriniferi gorged with imperfectly developed epithelium, which was mostly loaded to an unusual extent with oleo-albuminous granules."

6. All the other glands preserved their natural appearance, and their secretions were usually normal in character.

7. The intestinal canal was, not uncommonly, natural throughout in colour and appearance. In about two-thirds of the cases, however, there was what the French describe as "*psorenterie*," a prominence of the aggregated and solitary glands, but especially the latter. Patches of ecchymosis were also frequently found, especially in the cœcum.

8. The contents of the intestinal canal resembled in appearance the characteristic cholera stools. They differed, however, from the latter, in invariably containing immense quantities of perfect epithelium, which had become detached from the mucous membrane by maceration after death.

In the Medical Gazette for November 16, 1849, at p. 844, T. J. Herapath, Esq., of Bristol, gives the following as the results of his investigations into the *chemical composition* of the blood and discharges in cholera. He says,]

It will be seen that the venous blood in cholera differs from the healthy fluid—*first*, in containing a smaller amount of serum, and a larger proportion of globules and albumen; *secondly*, in being deficient in fibrin, and the inorganic salts, more particularly in those which are present in the serum; and *thirdly and lastly*, in containing a vastly abnormal proportion of urea, uric acid, and the constituents of the bile. It will also be found, 4. that the rice-water evacuations, and in fact all the excretions in this disease, are invariably alkaline. 5. That the alkalinity depends partly upon the presence of carbonate of ammonia, and partly, also, in the case of the dejections, upon that of the carbonates of potash and soda. 6. That the proportion of carbonate of ammonia existing in the excretions increases in a direct ratio to the duration of the disorder. 7. That the fæcal discharges in cholera differ from the healthy fæces, besides in the above respects, in containing a much larger comparative proportion of the soluble inorganic salts, and also of the ammonio-phosphate of magnesia, and the oxalate of lime.

II.—Microscopical Appearances of the Evacuations.

[Dr. Gairdner gives the following description of the cholera stools:]

These discharges separate by filtration, or on standing, into a colourless or slightly coloured fluid ,of an alkaline reaction, and a flaky sediment. The former is usually of a specific gravity, from 1005 to 1010, and contains, therefore, a very small proportion of solid matters; these have been shown by Dr. Parkes to consist chiefly of salts. Repeated examinations of this fluid have shown, during the late epidemic, (as Andral showed in the former), that albumen, as tested by heat and nitric acid, is not necessarily present in it. Indeed, the presence of albumen usually coincided with that of a small quantity of blood, which is frequently present. The fluid, however, contains *constantly* a small quantity of an organic substance which is precipitated by alcohol, by corrosive

sublimate, and (when acidulated) by ferrocyanate of potash; and which presents, in other respects, the chemical reactions of mucus.

The flakes which form the sediment of the cholera stools, have likewise the ordinary chemical reactions and physical properties of mucus. They are, however, opaque and turbid, and when submitted to the microscope, show a hyaline finely-striated basis, involving numerous granules, nuclei, and cells. The granules and minuter molecules require no particular description. The nuclei are from 1-250th to 1-180th of a line in diameter, circular or slightly oval, and not affected by acetic acid. The cells are comparatively few in number, mostly globular, seldom exceeding 1-150th of a line in diameter. Some of them are but little affected by acetic acid, and single-nucleated; others present all the appearances of perfect pus cells, being two, three, or even four-nucleated, and having the cell-wall rendered very transparent by acetic acid. The most common appearances, however, were the nuclei without cells, above mentioned.

[Respecting the *microscopical appearances* of the cholera evacuations, however, there has been no little discussion. In the beginning of July last, Dr. F. Brittan and Dr. J. G. Swayne, of Bristol, first saw in rice-water dejections, certain bodies which they regarded as peculiar and characteristic. The first description of these bodies appeared in the Medical Gazette of Sep. 28th, p. 530. Dr. Brittan there states that what he terms the "annular bodies." were found by him, generally in abundance, in the rice-water dejections and vomited matters from cholera patients, and also in the stools of severe choleraic diarrhœa; while he could not meet with them in healthy solid motions, nor in the fluid stools of typhus and other diseases. He found similar bodies in fluid condensed from the atmosphere of places where cholera was rife; while in situations free from cholera, they were sought for in the atmosphere in vain. The only difference between the "annular bodies" found in the atmosphere, the vomited matter, and the dejections, was in size; and as the sizes were progressive, it was concluded that the "annular bodies of atmosphere, vomit, and dejection, were but the three stages of development of one and the same body." Other peculiar bodies were also met with, but much more rarely—oval cells, containing a ball of granular matter, like a vitellus,—regarded by Mr. Swayne as the perfect cholera-cells. Specimens procured from the atmosphere, choleraic vomit, and evacuation, were sent by Dr. Brittan to Mr. J. Quekett, who pronounced them to be "successive stages of development of the same body," which he believed to be "of a fungoid nature." In the London Journal for November 1849, at p. 1051, Dr. Swayne gave a minute account of the cholera evacuations, including a description of the cells or bodies in question. The following are extracts from his description.]

Cholera evacuation chiefly differs from healthy fæcal matter in the following points: 1. The great bulk of the evacuation consists of a thin serous fluid, in which are floating a large quantity of mucous globules, mixed with a hyaline basis. 2. It contains little or

none of the amorphous matter tinged with bile. 3. It contains the same *débris* of animal and vegetable matters from the food, but in less quantity. 4. It contains much less epithelium than healthy fæcal matter. 5. It often presents a large number of crystals, which form the ordinary constituents of urinary deposits, especially triple phosphate, and lithate of ammonia; more rarely, lithic acid and oxalate of lime. It contains little or no chloride of sodium. 6. The most important difference is, that it presents those cells to which I have so often alluded, and which I have never detected in healthy evacuations.

These cells vary very much in size and apparent structure during the different stages of their development. The smallest are of the same size as, or even much less than, blood-globules; so that, to show them properly, an object-glass of high magnifying power, such as one-eighth, one-twelfth, or one-sixteenth of an inch is required. They are very transparent, and, like blood discs, appear to be flattened cells; but the thickness of their walls causes them to resemble rings in appearance. Their interior is almost entirely destitute of granules. Their walls refract light powerfully; they sometimes present a clotted or even cellular appearance, and there is usually a transverse fissure or crack at some point of their circumference. In some of them, I have observed very minute cells or buds, projecting at different points of their circumference. Fragments of them present the appearance of small segments of circles. Cells of such dimensions are most usually found in the first portions of the alimentary canal, especially in the matters ejected by vomiting. I have, however, found them present in large numbers in the fæces, together with other cholera cells of much larger size. These small cells precisely resemble, and are, in fact, identical in appearance with, those which Dr. Brittan has discovered in the atmosphere.

The medium and large-sized cells (as usually found in cholera evacuations) distinctly resemble the small cells in appearance, but they are coarser and more granular in structure. Between the three, every gradation may be met with, both as to form and size. The medium cells appear like thick, but somewhat irregular rings; but, on altering the focus of the microscope, they can be observed to be flattened cells, with granular contents, and in some cases containing distinct cells within them. Their walls appear thick, and cellular in structure, the arrangement of the cells sometimes giving them the appearance of transverse striæ. The walls have usually a great tendency to split at four or five points of their circumference; and, when split by gentle pressure, the whole cell divides into four or five fragments, and gives exit to its granular contents.

The larger cells (as usually met with) are more irregular in shape, and cellular in structure. They are semi-opaque, of a dirty-yellow colour, and have lost much of their resemblance to rings. On bringing their surface into focus, three or four cracks can usually be seen upon it, which appear deep fissures when viewed in profile. The cell wall is distinctly cellular in structure. The cell

often contains within it other cells of a similar nature. When one of these large cells is crushed by pressure, it breaks into a great number of fragments, of a round cellular form. Sometimes, however, the fragments have an angular character.

A very short time ago, I had an opportunity of examining the only perfect specimen of these cells which I have seen. In one of my earlier examinations I met with a large, well-developed cell, but this did not perfectly reveal the structure, and it is only lately that I have seen any which do this. Their walls were thick, and studded externally with numerous cells or buds. These appeared to be arranged in concentric circles. The cells being somewhat globular, the centre is first brought into focus, when it is seen to be occupied by two or three of these buds, with intervals between them. On gradually bringing the rest of the cell into focus, wider and wider circles of these buds are successively displayed, until the outer margin or ring of buds is brought into focus. The cells forming these circles are connected by very distinct concentric lines.

The parent cell is seen to contain within it a mass of granules, which in imperfectly developed cells, does not quite fill its interior. On crushing one of the parent cells, it gives exit to its contents, consisting of granular matter, somewhat resembling the most minute cholera cells in appearance. Well-developed large cells of this kind are usually tolerably transparent, and of a dirty-yellow colour.

[Mr. Busk, the president of the Microscopical Society, has denied (Medical Gazette, Oct. 19th, p. 692,) that the bodies described and exhibited by Drs. Brittan and Swayne, were new or peculiar. He has affirmed that the "perfect cholera-cells" of Mr. Swayne were nothing more than a species of *uredo*, a kind of smut frequently found on wheat, and specimens of which he found in a loaf of brown bread purchased at Greenwich: and likewise that the so-called "annular bodies" were merely altered starch granules, or else portions of the cells from the inner coating of bran.

Dr. W. Baly and Dr. W. W. Gull, the cholera committee of the College of Physicians, after making a minute and searching experimental inquiry into the facts stated by Drs. Brittan, Swayne, and Budd, have published a report of their investigations (Medical Gazette, Nov. 2, p. 775). They state that the principal results at which they have arrived, are the following:]

"1. Bodies presenting the characteristic forms of the so called cholera fungi are not to be detected in the air, and, as far as our experiments have gone, not in the drinking-water of infected places.

"2. It is established that, under the term 'annular bodies' and 'cholera cells or fungi,' there have been confounded many objects of various and totally distinct natures.

"3. A large number of these have been traced to substances taken as food or medicine.

"4. The origin of others is still doubtful; these are clearly not fungi.

"5. All the more remarkable forms are to be detected in the intestinal evacuations of persons labouring under diseases totally different in their nature from cholera.

"Lastly. We draw from these premises the general conclusion that the bodies found and described by Messrs. Brittan and Swayne are not the cause of cholera, and have no exclusive connection with that disease; or, in other words, that the whole theory of the disease which has recently been propounded is erroneous, as far as it is based on the existence of the bodies in question."

[Mr. T. J. Herapath, of Bristol, while allowing the existence of certain peculiar bodies in the cholera evacuations, denies their constant occurrence in the air and water of infected neighbourhoods, and thinks that the cellular character of the bodies is not sufficiently evident. He says that, though they possess many of the outward appearances of a cellular organism, their behaviour with re-agents is of a different character. It is well remarked by Mr. Herapath that:]

If it is possible to be proved indisputably, only in one instance, that a case of cholera may occur at the same time that these cells are absent both from the water and from the air, it is evident that we must look in some other direction for the *fons et origo mali;* or, at all events, if they are truly the cause of the disease, then they must be capable of entering into the human organism by some other means than we are at present acquainted with.

[Dr. N. Parker, Lecturer on Microscopical Pathology at the London Hospital, observes, in a paper published in the Medical Gazette, Oct. 19th, p. 671, that if these bodies do exist, and even if they are of a fungoid nature, it does not therefore follow that a causal relation obtains between these fungi and the diseased state. The following considerations lead him to believe that such a relation does not obtain:]

1st. The active principle of the *fixed* vehicles of contagion cannot be proved to be a fungus, and therefore analogy would lead me to predicate the same respecting that of contagious atmosphere.

2nd. Even were fungi present, they must be proved to be *essential*, and not accidental; the cause of the disease, not its symptom; for fungi are present in various and most different states of the economy, often without giving rise to any particular symptoms.

3rd. But these bodies do *not* seem to be invariably present in the evacuations in cholera: they cannot, therefore, be its cause.

4th. The presence of a few fungi does not serve in any way to explain the terrible symptoms of cholera. Nor is our knowledge of the essential nature of this mysterious affection thereby materially advanced.

[It is necessary to state that Dr. Swayne has since (Med. Gazette, Nov. 16, p. 860) replied to Mr. Bush's observations, giving an elaborate account of the differences between his "cholera-cells" and

"annular bodies," and the uredo and starch granules which Mr. Bush supposed them to be. Dr. Swayne still maintains the peculiar and distinctive nature of these bodies.

Of the appearances seen by other observers, there is no need to say much. In the Medical Gazette for Oct. 5th, p. 579, Dr. T. Williams, of Swansea, gives an account and engravings of the appearances which he saw in rice-water evacuations. The peculiar bodies which he observed, consisted of flattened polygonal (for the most part pentagonal) corpuscles, the interior of which was incompletely filled by a mass of molecules aggregated in a spheroidal shape, and presenting in the centre a pellucid nucleolar point. Can these be identical with the perfect "cholera-cells" of Dr. Swayne? M. Pouchet states (Medical Gazette, Sep. 28, p. 545) that he has found vibriones in the rice-water dejections; and Dr. Waldo Burnett, of Boston, U.S., has found them in great abundance, not only in the stools, but in the substance of the muscles. (London Journal, Oct. 1849, p. 950). When this statement was published, Dr. Burnett had not, unfortunately, had an opportunity of repeating his observations.

III. Causes of Cholera.

[If the intimate pathological nature of cholera is uncertain, no less are its causes involved in obscurity. Abundance of speculation has been hazarded upon the subject, but the discussion to which it has given rise has hitherto been barren of any very practical result. Our space will only allow us to refer briefly to some of the papers which have been written upon the subject, since the publication of our last volume.

Electrical Theory.—Certain facts connected with the history of the late epidemic at Paris, (for which see Lancet, July 28, p. 94, and Medical Times, June 23, p. 659) show that there is at least a very striking coincidence between the amount of electricity in the air, and the virulence of the cholera epidemic. In a letter to the President of the French Academy, bearing date, June 10th, 1849, M. Andraud stated that he had been making daily experiments upon the action of the electrical machine, for the previous three months. He said]

The machine I have used for my daily observations is rather powerful; in ordinary weather it gives, after two or three turns of the wheel, brilliant sparks of five or six centimetres. I have remarked that since the invasion of the epidemic I have not been able to produce on any one occasion, the same effect. During the months of April and May, the sparks, obtained with great trouble, have never exceeded two or three centimetres, and their variations accorded very nearly with the variations of the cholera; this was already for me a strong presumption that I was on the trace of the important fact that I was endeavouring to find. Nevertheless, I was not yet convinced, because one might attribute the fact to the moisture of the air, or to the irregularities of the electric machine. Thus I waited with impatience the arrival of fine weather and

heat, to continue my observations with more certainty. At last fine weather, and, to my astonishment, the machine, frequently consulted, far from showing, as it ought to have done, an augmentation of electricity, has given signs less and less sensible, to such a degree, that during the days of the 4th, 5th, and 6th of June, it was impossible to obtain anything but slight cracklings, without sparks. On the 7th the machine remained quite dumb. This new decrease of the electric fluid has perfectly accorded, as is only too well known, with the renewed violence of the cholera: for my part I was not more alarmed than astonished—my conviction was complete. I saw only the consequence of the fact already supposed. It may be imagined with what anxiety, in these moments of the crisis, I consulted the machine, the sad and faithful interpreter of a great calamity. At last, on the morning of the eighth, some feeble sparks reappeared, and from hour to hour intensity increased. I felt with joy that the vivifying fluid was returning in the atmosphere. Towards evening a storm announced at Paris that the electricity had re-entered its domain; to my eyes it was the cholera which disappeared with the cause which produced it. The next day, (Saturday, the 9th,) I continued my observations; the machine at the least touch rendered with facility some lively sparks.

[Now it is stated that in the six days following the 8th of June, *the mortality in Paris fell gradually from* 667 *to* 355. Another very curious circumstance is related by Dr. KIDD, of Limerick. While the epidemic was raging in that city, a large magnet, capable of sustaining seventy pounds, was watched daily. No variation took place in it. But on the appearance of the disease in Dublin,—100 miles east of Limerick,—the cholera being quite gone from the latter place, the magnet quite suddenly lost nearly all its power. (Medical Times, July 14, p. 25.)

Deficiency of Ozone in the Atmosphere.—This theory has been advocated by Mr. R. HUNT. (Lancet, Sept. 11, p. 473.) It is true, apparently that there has been less ozone than usual in the atmosphere of London: but a much larger number of facts bearing on this point must be collected before any just induction can be drawn.

Poisoned Water.—During the late epidemic, the following circumstances occurred:—At a row of houses in Wandsworth, twenty persons of the middle class of life were successively cut off by cholera in a few days. As, by the bursting of some drains, owing to the heavy rains a few days before, the water used by the inhabitants had become contaminated by admixture with the cess-pool water, it was supposed by some that the patient had died from drinking poisoned water. Dr. JOHN SNOW adopted this explanation. (Medical Gazette, Sep. 21, 1849, p. 504.)

Dr. LLOYD, also, as will be seen by referring to p. 431 of the present volume, ascribes the occurrence of some cases to a similar cause.

Fungous Origin.—Dr. T. K. MITCHELL, of Philadelphia, after showing (Medical Gazette, Oct. 5th, p. 602,) that there are certain

points in the history of the march of cholera which cannot be explained adequately in any other theory, says]

But if we assume for cholera a fungous origin, all difficulties vanish; and, as in the case of yellow fever, an easy explanation may be given of every apparent incongruity. We have only to suppose, what is known to happen in other cases, that the fungi, on which cholera is assumed to depend, acquire at times, as do the germs of some contagious diseases, an unusual power of reproduction and diffusion, a greater potency of expansion. Such germs may be carried by men, and goods, and ships, and may make a slower progress by their own unaided activity, or be scattered by the winds to regerminate wherever special conditions are to be found. Thus can we see why the poison prefers the route of streams, or infests the damp parts of cities; and why classes living in clean apartments in dry districts suffer so little.

[*Specific Morbid Poison.*—The theory which ascribes the production of cholera to a specific morbid poison, which is reproduced in the body of each person affected with the disease,—involving, as it does, the difficult and important questions of *contagion* and *quarantine*,—has given rise to much discussion. Facts in great abundance are brought forward by both contagionists and non-contagionists, in support of their respective opinions. And both classes of facts force themselves, more or less, upon the notice of those who have no particular theory to support: as an example of which we may refer the reader to pp. 431-2, of the present volume. Indeed this question, as an anonymous writer (Medical Gazette, Oct. 5, p. 600.) well observes]

Scientifically considered, appears hardly susceptible of a rigorous solution. Every case of seizure following on communication with an infected person may be met with a case, equally authentic, of seizure following on arrival in an infected place, or on respiration of contaminated air. On the other hand, every case of escape, notwithstanding exposure to effluvia emanating from the sick, may be counterpoised by equivalent instances of escape notwithstanding residence in a contaminated district, or immersion in a contaminated atmosphere. The rejoinder, founded on a presumed lack of predisposition in the individual exposed, is equally available on both sides of the question; nor can it ever be certainly known, with respect to either of the assigned causes of infection, whether it did or did not co-operate with some other; nor to which of several influences, all possible, perhaps all coexistent, the observed effect is due,

[We believe, however, that wider generalizations than we at present possess, may advantageously be made, and we wish that some of the accomplished physicians of the present day would enter into a close and candid examination of all the evidence collected on the subject. In the mean time, however, it is interesting to know, that, in the words of the writer above quoted,]

Practically, the question is conclusively settled in the non-contagionist sense. The experience of our own hospitals and hospital ships (especially the Dreadnought and the Dover) has proved that *the physicians and nurses of cholera patients are not more liable to the distemper than persons not so engaged.* And the experience of Naples, Vienna, Moscow, and other continental towns, has proved, beyond dispute, the utter inefficacy of quarantine regulations to repel epidemic invasion.

A SYNOPSIS,

CONTAINING

A SHORT ABSTRACT OF THE MOST PRACTICAL ARTICLES IN THE FOREGOING PAGES OF THIS VOLUME; AND SHOWING, AT A GLANCE, THE MOST IMPORTANT INDICATIONS OF TREATMENT PUBLISHED BY DIFFERENT WRITERS WITHIN THE LAST HALF-YEAR. (ARRANGED ALPHABETICALLY.)

DISEASES AFFECTING THE SYSTEM GENERALLY.

ANÆMIA.—Give nourishing diet, with as much animal food as the patient can bear: let the patient be exposed "to the pure air and light of heaven as free and as long as the strength and sensibility will bear"; promote the natural excretions by appropriate means: and give tonics, especially iron, the syrup of the iodide, preceded or not by some mild bitter. Employ diffusible stimulants, sedatives and narcotics, external stimulants, rest in the horizontal posture, and warmth to the extremities, when special circumstances point out the necessity for these means. And if the nervous symptoms do not cease with the removal of the anæmia, give the metallic tonics, zinc, silver, and copper. (Dr. C. J. B. Williams, p. 23.)

In those cases of anæmia which are not benefited by iron, give salts of manganese. The carbonate or phosphate may be given in the form of pill, to the extent of three or four grains daily; or the phosphate or iodide may be given in the form of syrup, according to the formula at p. 371. (M. Hannon, p. 25.)

CACHEXIA.—In scrofulous, syphilitic, or cancerous cachexia, manganese has been recommended. (See "*Anæmia.*")

DROPSY.—Give wine of colchicum, beginning with ʒij. daily, and increasing the dose by about ♏xx daily. When thus given it is a powerful hydragogue. (M. E. Vandezande, p. 363.)

Following Scarlatina.—Give a brisk purgative, and follow it by the liberal use of antimony, and the antiphlogistic regimen. In some cases leeches may be applied over the region of the kidneys. (Dr. Newbigging, p. 13.)

Let a flannel dress be worn, the warm bath be used occasionally, and antiphlogistic treatment employed. In the advanced stages, give tinct. ferri sesquichlor. (Dr. Hughes, p. 21.)

Give mild diuretic salts, as the acetates of potash and soda, with spirit of nitric ether; and use warm baths, and let the skin be rubbed with oil of turpentine. Take especial care that the child is kept wrapped in flannel and in a warm atmosphere. If the child continues pale and weakly for some time, give iron, and recommend change of air. In severer cases, from six to ten leeches must be repeatedly applied to the lumbar regions. (Dr. F. J. Behrend, p. 21.)

Fever.—*Intermittent.*—Begin by purging freely, with calomel and jalap, followed by senna and salts. Then, after the first paroxysm of the fever has clearly declared itself, give tartarized antimony in such doses as to prostrate the patient's strength, and keep up its effects for some time, during all stages of the fever, without producing any violent effects on the stomach or bowels. The dose required for this purpose, in an adult male, will usually be one-tenth of a grain repeated every hour; but the quantity must be changed, and the interval lengthened or shortened according to circumstances. In cases complicated with congestion or inflammation of the internal viscera, local and general bleeding must be employed to aid the effects of the antimony. And in protracted cases, where the system appears to become habituated to the remedy, the tartar emetic should be discontinued, and twelve or fifteen ounces of blood quickly abstracted by cupping from the epigastric and hypochondriac regions; five grains each of calomel, and James's or antimonial powder should be given at bed-time, and compound jalap powder in the morning; and after the operation of the purgative, if a fresh paroxysm of the fever distinctly declares itself, the tartar emetic should be resumed. (Mr. Moore, p. 6.)

In the depraved state of the blood which follows ague, give manganese, which will be found the most certain remedy for preventing a return of the attacks. The carbonate, malate, or phosphate may be given in the manner described at p. 371. (M. Hannon, p. 373.)

Typhus.—When the petechiæ are very dark, or threatening to run into putridity, or in cases of dysentery attended with great fœtor of the dejecta, give yeast in the following manner:—℞. Cerevis. fermenti, ℥x.; camphoræ ʒss.; æth. nitric., ʒiv. M. An ounce every one, two, or three hours. (Dr. J. Lamprey, p. 9.)

In many cases of collapse occurring in fever, transfusion of blood might properly be used; as in the exhaustion which occurs in a variety of Irish fever, the crisis of which is marked by excessive perspiration; and also in a more advanced stage of the convalescence, when death is imminent from simple weakness. (Dr. C. H. F. Routh, p. 153.)

In the *diarrhœa and dysentery* of fever, give oil of turpentine in doses of eight minims, in the form of emulsion, and, if the case is tedious, change the form of exhibition, by giving Chio turpentine in pills. In some cases the the administration of mucilage and

sedatives, together with the application of leeches and blisters over the cœcum, is useful, and so also is the application of mercurial ointment in the same region; but mercury, given internally, and the mineral astringents given either by the mouth or by enema, are inefficacious. (Dr. Hudson, p. 10.)

Gout.—Small bleedings to the extent of from three to six ounces, are useful. When purgatives are used they should be the warm cordial ones, which may be advantageously given in the form of tincture, and associated with aromatics and bitters. Colchicum should not be given to the extent of producing vomiting or purging, as it will act quite as well without any such manifestation. To change the uric acid condition of the urine, give alkaline phosphates, tartrates, or citrates, rather than carbonates, or pure alkalies. (Dr. W. Gairdner, p. 30.)

Give lemon-juice, in doses of from half-an-ounce to an ounce three times a day. (Dr. G. O. Rees, p. 31.)

The best aperient in cases of gout, consists of ten grains of *pure* sulphur, and ten grains of sulphate of patash, with occasionally five grains of rhubarb. (Anon. p. 31.)

Rheumatism, *Acute.*—Give lemon-juice in doses of half-an-ounce or an ounce thrice daily. There is no advantage derived from continuing the remedy when debility remains after the subsidence of the inflammatory symptoms. (Dr. G. O. Rees, p. 31.)

Give five grains of pil. sapon. c opio, night and morning, and the acetate of potash in a mixture with sugar, water, and essence of lemon, during the day; and use vapour-baths. (Dr. Golding Bird, p. 33.)

Give phosphate of ammonia, in doses of from ten to thirty grains, in cinnamon water. (Dr. Pepper, p. 34.)

An anæmic condition may supervene in the course of acute rheumatism, resulting, apparently, rather from the natural course of the disease than from the remedies employed. Chalybeates should be given in this condition. (Dr. O'Ferrall, p. 24.)

Chronic.—Apply the heated "hammer." See "*Neuralgia.*"

Ricketts.—[In some excellent papers printed in the present volume, Mr. E. F. Lonsdale details the mechanical treatment of ricketts and other curvatures of the legs. It is impossible to condense to any extent the excellent directions given by Mr. Lonsdale, without at the same time rendering them obscure: we must therefore refer our readers to the articles themselves, at pp. 116—125.]

Scarlatina.—After an aperient (which should be repeated towards the disappearance of the eruption), give antimony freely, employ tepid sponging, and regulate the diet carefully. Apply lunar caustic, either solid or in solution, to the throat at an early period, and repeat it frequently, and let water acidulated with nitric acid be used as a gargle. (Dr. Newbigging, p. 13.)

Apply a thick flannel pad, wrung out of hot water till almost dry, upon the pit of the stomach and over the region of the heart, and then wrap the patient carefully in several blankets. At the same time wrap another flannel, wrung out of hot vinegar and water, round the throat. After the patient has *perspired* profusely for three or four hours, remove the flannels carefully, and give Stevens's saline powder (to an adult half a drachm) every four hours: and let him use a gargle containing ℈ij. of burnt alum, ℈ij. of Armenian bole, and ʒj. of brown sugar in ℥viij. of water. (Mr. F. A. Bulley, p. 19.)

Take a piece of bacon, the size of the hand, make slits in various directions on the soft side of it, and place it in a warm place to promote the oozing out of the fat. After allowing the bacon to cool, rub the patient with it, morning and evening, diligently over the whole body, remembering to have only one part uncovered at once. Keep up the rubbing twice a day (in severe cases three or four times) for three weeks, and once a day during the fourth. After this, wash the patient daily with cool water and soap, and afterwards the warm bath may be used. (Dr. Schneeman, p. 16.)

Prophylactic Treatment of Scarlatina.—Give belladonna, from one-sixth to one-fourth of a grain twice a day, according to the age of the child,—the first dose before breakfast, and the last at bedtime. The use of this remedy may be continued for several weeks. (Dr. Newbigging, p. 15.)

Give belladonna, in the following manner: take of extract of belladonna, one to two grains; distilled water, one ounce. Mix. Give to each child, morning and evening, as many drops as it has years, and continue its use for at least fourteen days. (Dr. Schneeman, p. 18.)

AFFECTIONS OF THE NERVOUS SYSTEM.

Lumbago.—Apply the heated "hammer." See "*Neuralgia.*"

Neuralgia, *Facial and Frontal, and other forms.*—Use counter-irritation by means of a small iron "hammer" gently heated. Heat the instrument in the flame of a spirit lamp for fifteen or twenty seconds, and then run it lightly, alternately raising and depressing it, over the place affected. (Dr. G. E. Day, p. 51.)

Neuroma, *or Tumour of Nerve.*—No measure is successful except that of excision. The tumour may either be dissected out of the mass of nerve-fibres among which it has grown, or it may be cut away together with the piece of nerve to which it is attached. The latter measure is generally quite safe and practicable. Lastly, amputation may be resorted to (if the disease is situated in an extremity), when other means fail, and the symptoms are urgent. (Dr. R. W. Smith, p. 60.)

Paralysis, *Facial.*—In facial paralysis from exposure to cold, apply the "heated hammer," a modification of the iron recommended by Dr. Corrigan, which is to be heated for fifteen or twenty seconds in the flame of a spirit lamp, and then run lightly, being alternately raised and depressed, over the affected surface. (Dr. G. E. Day, p. 52.)

Paraplegia.—The long-continued use of small doses of bichloride of mercury, in solution, has been found beneficial. (Dr. J. C. Hall, p. 227.)

Sciatica.—Apply the heated "hammer." See "*Neuralgia.*"

Tetanus, *Traumatic.*—When tetanus is threatened in consequence of a punctured wound, convert the latter into an incised wound by laying it freely open to the same depth as the original puncture; and if it is then found that a branch of nerve had been punctured or partially divided, cut it completely through. The plan of cauterizing the wound with a view to promote suppuration, is unadvisable. Amputation is not generally advisable, after tetanic symptoms have set in; there are cases, however, in which it may be resorted to, especially when the injury is so severe, that the limb is not likely ever to be a useful one. (Mr. B. Cooper, p. 130.)

A case of tetanus coming on when a wound was nearly healed, was cured after destruction of the cicatrix by means of a red-hot iron. (M. Remy, p. 61.)

AFFECTIONS OF THE RESPIRATORY ORGANS.

Bronchocele.—In some cases, where surgical interference seems called for, the seton will be a legitimate and proper mode of treatment. In performing the operation, do not use a common seton needle, but get a curved trocar made, fixed in a handle, grooved like a lithotomy staff, and having an eye behind the shoulder. Having threaded this eye with a single silk, which has a skein attached, enter the trocar at the upper part of the tumour in the median line, carry it on in a semicircular direction, so as to embrace a good deal of the tumour, and bring out the point in the median line below. Get hold of the silk thread, then withdraw the trocar, and pull the skein through. (Mr. R. Hey, p. 344.)

Hooping-cough.—Give strong coffee, well sweetened. The dose for a child of two years old is a teaspoonful four times a day, or oftener; for a child of four years, a dessert-spoonful; and for an older patient, a tablespoonful. Let the diet consist principally of fried and roast meat; the quantity of milk used being diminished, and farinaceous food, confectionary, and fruit, being entirely prohibited. (Dr. Jules Guyot, p. 90.)

Larynx, *Ulceration of.*—The application of solution of nitrate of silver is very useful, but the difficulty is to get it applied. Perhaps if the sponge saturated with the solution, instead of being fastened to a piece of whalebone, were fastened to a pervious hollow tube, it would answer better, as allowing free respiration. A large elastic male catheter, with a wire bent to the proper curvature, might answer the purpose. (Mr. J. Bedingfield, p. 88.)

Phthisis *Pulmonalis.*—Manganese has appeared to be very useful. The syrup of the iodide appears to be the best form of administration in these cases, but the carbonate, phosphate, or sulphate may also be given. Formulæ are given at p. 371. (M. Hannon, p. 26.)

Give cod-liver oil in every stage of the complaint. The experience at the Brompton Hospital shows that this remedy "possesses the property of controlling the symptoms of pulmonary consumption, if not of arresting the disease, to a greater extent than any agent hitherto tried." (p. 370.)

The taste of cod-liver oil may be disguised by masticating a morsel of dried orange-peel just before and just after swallowing the dose. (M. Fredericq, p. 371.)

It is more than probable that any fish oil, southern whale oil for instance, would be as efficacious as cod oil. The experiment should be tried on a large scale. (Mr. R. Druitt, p. 368.)

Trachea, *Wounds of.*—When the larynx or trachea has been freely divided, first attend to the suppression of the bleeding. When this is effectually done, bring the parts accurately together by sutures, introduced even through the cartilages, if necessary. If very great difficulty of breathing should afterwards occur, threatening asphyxia, open the trachea longitudinally. (Mr. A. Eves, p. 347.)

Tracheotomy.—Do not operate for croup or laryngitis in children if the ribs are motionless during inspiration, as this indicates that effusion has taken place into the pulmonary tissues. If the lungs were free, the ribs would be pressed inwards or backwards by the external atmospheric pressure, when attempts were made at inspiration while there was some serious obstruction to the air passages. (Dr. G. O. Rees, p. 89.)

AFFECTIONS OF THE CIRCULATORY ORGANS.

Aneurism.—[A new instrument for the treatment of aneurism by compression, is described by Dr. Carte, p. 137.]

By Anastomosis.—See "*Nævus*"

False, of the Bend of the Elbow.—Apply a compress over the aneurismal sac, letting it extend upwards in the course of the artery, and carefully bandage the entire limb, beginning with the fingers

and thumb, and extending to the axilla; and let the limb be kept elevated on an inclined plane, and perfectly quiet. If this does not succeed, cut into the sac, and tie the artery above and below; or, what is better, tie the artery above the tumour, and then, the impetus of the blood being taken off by the ligature, employ slight pressure upon the tumour. (Mr. John Adams, p. 137.)

Aneurismal Varix.—Tie the brachial artery in the middle of the upper arm, and apply gentle compression over the tumour, and in the course of the radial and ulnar arteries. If this plan fails, the following operation, a much more dangerous one, must be performed: place a tourniquet on the brachial artery, so as completely to compress it, and then make a free incision into the aneurismal sac; turn out the clot of blood, and seek for the upper and lower openings of the artery. Pass the end of a probe into the upper opening, to serve as a guide in separating the artery from the surrounding parts, and secure the vessel: then repeat the same process upon the distal end. (Mr. B. Cooper, p. 144.)

Arteries, *Wounds of.*—Many arteries should be tied at once when wounded, but this does not apply to all. When the brachial artery is wounded in venesection, squeeze the edges of the wound accurately together, and place over it a sixpence or similar firm body, rolled up in a piece of soft rag: then apply something to press upon the radial, ulnar, and humeral arteries, a little below and a little above the wound, and apply a bandage. Bandage the arm to a splint, and keep the patient perfectly quiet. By this means, the wound of the artery will sometimes heal; at any rate the wound in the integuments will, and then, if aneurism form, it can be treated subsequently. (Mr. John Adams, p. 136.)

In deep punctured wounds of the palm, where it is wished to stop the bleeding by compression, apply a bandage round each finger separately, and then continue it round the hand, placing a compress immediately over the wound. Continue the bandage up the forearm, having placed small compresses of cork over the radial and ulnar arteries, and make sufficient pressure to diminish, without wholly stopping, the supply of blood. Lastly, carry the bandage with a moderate degree of tightness up to the shoulder, keep the patient recumbent, and apply cold to the arm. (Mr. B. Cooper, p. 139.)

Hemorrhage.—In all cases of collapse induced by hemorrhage, whether primary or secondary, transfusion of blood should be employed. (Dr. C. H. F. Routh, p. 152.)

After Extraction of Teeth.—In severe cases use the actual cautery. It is best applied by means of a small rod of polished iron, furnished with a sheath; the end of the sheath is applied to the bleeding part, and the heated iron then inserted, and passed along the sheath until it reaches the part. (Mr. B. Cooper, p. 138.)

Nævus.—When the tumour is small, it is often sufficient to keep the surface constantly wetted with a strong solution of alum; or a saturated solution of alum may be injected into the tumour with a very fine syringe, but this must not be done if the tumour is large. Pressure, by means of a plaster cast bound upon the part, is only applicable when the disease is situated over bone, and is not always successful; neither is vaccination. The application of nitric acid is objectionable, from the hemorrhage which results when the slough separates; the same danger, that of hemorrhage, attends the practice of excision. The ligature is the most generally applicable mode of treatment, and may be applied, if the tumour is small, by passing two needles through its base at right angles to each other, tying the ligature tightly round them, and replacing it by another when it becomes loose; if the tumour is large, another method, Mr. Liston's, which will be understood by a reference to the figure in the text, should be employed. (Mr. B. Cooper, p. 145.)

Apply a solution of iodine ℈i., in sp. vin. rect. ℥ss., freely once every day. (Dr. J. O. Bulteel, p. 147.)

Phlebitis.—The principles of treatment are to promote the secretions without lowering the strength, and to allay irritation. For these purposes give a combination of calomel, antimony, and opium, and a mixture containing liq. ammon. acet., tinct. of hyoscyamus, and camphor mixture. Keep the limb elevated, apply fomentations and poultices over the whole limb, and make punctures through the skin wherever it has become tense from effusion; but do not apply leeches. When the local irritation has been thus subdued, give bark, mineral acids, porter, and nutritious food, and continue them without fear so long as the natural secretions are duly performed. (Mr. B. Cooper, p. 65.)

Transfusion of Blood.—[Minute directions for performing this operation are given by Mr. Walton, p. 154.]

[Instructions for injecting substances into the veins are also given by Dr. M'Gregor, p. 428.]

Veins, *Admission of Air into.*—Compress the wounded vein; place the patient in the recumbent position, and dash cold water in his face; support respiration by artificial means; apply warmth and stimuli, such as sinapisms &c., to the surface of the body; hold ammonia to the nose; and give it internally mixed with wine, if the patient be capable of swallowing. (Mr. B. Cooper, p. 64.)

AFFECTIONS OF THE DIGESTIVE ORGANS.

Anus, *Fissures of.*—Having introduced the left fore-finger into the rectum, make a small opening at the verge of the anus, and through this wound introduce a straight knife, (Blandin's knife) guarded at its point and edges, into the submucous cellular tissue, between the mucous membrane and sphincter Turn the cutting edge towards the muscle, retract the guard, and cut through the

muscle as the blade is withdrawn. Then close the external wound, and keep the bowels lax by means of conf. sennæ. (Mr. W. J. Anderson, p. 172.)

Diarrhœa.—In cases where very great exhaustion occurs, transfusion of blood might probably be advantageous. (Dr. C. H. F. Routh, p. 153.)

Occurring in Fever.—Give oil of turpentine, in doses of six or eight minims, in the form of emulsion: or Chio turpentine in pills. (Dr. Hudson, p. 10.)

Premonitory of Cholera.—See "*Cholera.*"

Dysentery.—In cases where very great exhaustion occurs, transfusion of blood might probably be advantageously employed. (Dr. C. H. F. Routh, p. 153.)

In the Course of Fever.—Give oil of turpentine in doses of six or eight minims in the form of emulsion; or Chio turpentine in pills. (Dr. Hudson, p. 10.)

Tropical.—In *acute* tropical dysentery, give daily enemata of tepid water—four to six pints—by means of O'Beirne's elastic tube, introduced beyond the sigmoid flexure. This soothes the inflamed parts, and gradually softens and brings away the mass of hardened fæces which usually fills the large bowel in this disease. After the intestine has been freely cleared of the fæcal matter, the tube need not be introduced so far. Free venesection will often be required in acute dysentery. In the *chronic* dysentery, employ astringent injections, such as fifteen grains of nitrate of silver dissolved in two or three pints of water, or a mixture of catechu and chalk, or a strong decoction of bark; taking great care to wash out the bowels with a large enema of tepid water, (as above described) every day, before giving the astringent injections. Or, conjointly with the warm water enemata, astringent medicines may be given by the mouth. Leeches will often be required; but very seldom mercury, except in the form of ointment. (Mr. E. Hare, p. 98.)

Dyspepsia.—In obstinate cases, where there is great irritability, depression of spirits, and anxiety, examine the urine for oxalate of lime. If this substance is contained in the urine, give nitro-muriatic acid. (Dr. James Begbie, p. 91.)

In cases of extreme exhaustion, when the patient is likely to sink from inanition, employ transfusion of blood; it may enable the patient to rally, so that other remedies may subsequently take effect. (Dr. C. H. F. Routh, p. 153.)

Hernia, *Strangulated.*—Choose the operation by opening the sac. This practice does not increase the danger, as the peritoneum may be wounded with impunity, and peritonitis is a comparatively rare cause of death after hernia operations; while there are important advantages to be derived from exposing the intestine. (Mr. W. Hancock, p. 157.)

Always open the sac. Section of the peritoneum is not dangerous, but on the contrary, beneficial; and if the sac is not opened, strangulation will sometimes not be removed by the operation, the sac being returned as well as its contents. (Mr. B. Travers, Jun., p. 165.)

Always open the sac freely; and, after the operation, pass the finger into the abdomen, and examine it for some little space within the stricture, to be sure that the intestine is free. (Sir John Fife, p. 160.)

Never open the sac, except when compelled to it; for the object of the operation may be effected, nine times out of ten, by dividing the stricture external to the sac; the process is less severe and the result more successful than by the old method, and, with moderate care, there is no danger of pushing back the sac together with its contents. The objection that gangrenous intestine or omentum may thus be returned into the abdomen will hardly hold good: for when the contents of the sac are in this state, it is scarcely possible to return them without the exercise of such force as we know to be unjustifiable. And if a portion of intestine is in a *doubtful* state, i.e. not actually perforated, but likely to become so, the safest place for it is in the abdomen. After an operation for hernia, keep the bowels quiet for some days by means of opium; to which calomel may be added, as it maintains the healthy functions of the skin, liver, and kidneys, and renders it easy to unload the bowels by mild means. (Mr. Cock, p. 169.)

Always divide the stricture outside the sac in the first instance: the latter can afterwards be opened if found necessary. Do not make lamellar separations of the fascia, and do not wound more tissues or go deeper with the incision than is absolutely necessary; and especially avoid interference with the hernial tumour and its envelopes, if possible. Make a small incision, large enough to admit the finger, through the skin and superficial fascia on the inner side of the tumour; and through this, reach the seat of stricture and divide it, touching the sac only at the constricted point. (Mr. Gay, p. 166.)

Irreducible Omental.—Adopt every means to get it reduced; and if the patient cannot spare the time, or will not undergo the discipline necessary for starving out a portion of protruded omentum, and rendering it capable of being returned into the abdomen, recommend an operation. The ease and safety which attend Petit's operation, will justify us in doing this. (Mr. Cock, p. 168.)

Hemorrhoids.—Apply nitric acid for the cure of both external and internal piles. Give the patient an aperient before performing the operation,—a blue pill at bedtime, and a saline purge in the morning; and after the operation, desire the patient to remain in bed or on a sofa the rest of the day. Always advise people who have piles to defecate just before going to bed, and that they may do this, give a dinner pill, containing about a grain each of aloes, soap, extract of liquorice, and gum mastic. (Dr. R.T. Massy, p. 173.)

An instrument for at once cauterising and constricting the piles, is described by (M. Amussat, p. 175.)

External.—Apply potassa fusa, and repeat the application in a few days. (Dr. F. C. Jones, p. 175.)

Œsophagus, *Foreign Bodies in.*—If the body be small and sharp, as a needle or fish bone, use a large goose or swan quill feather, with the barbed portion ruffled, and imbued with oil. The patient's head being supported against the breast of an assistant, lower the tongue, introduce the feather with its concave side downwards into the throat, turn it rapidly round, and draw it out. If the body be large, use a rather straight lithotrite with an imperforate scoop. Introduce it with the blades closed, down to the foreign body, separate the blades sufficiently to grasp the substance, and after a few gentle turns, withdraw it. If the substance is a piece of flesh, and near the cardiac orifice, propel it onward to the stomach with a probang. For extracting coins, use the double ring of Graefe attached to the end of a rod of whalebone, with a steel spring. (Prof. Dieffenbach, p. 156.)

Stricture of.—In some of those cases where no food can be taken, employ transfusion of blood, to give the patient time to rally, so as to admit the introduction of bougies subsequently. (Dr. C. H. F. Routh, p. 153.)

Salivation.—Apply solution of nitrate of silver (ʒ j. in an ounce), to the gums, three or four times daily with a hair-pencil. The teeth will be discoloured, but the stain may be removed by a few applications of a solution of cyanide of potassium, (one part of the salt to two of distilled water.) (Mr. Kirby, p. 176.)

Use a gargle composed of half-a-drachm of kreasote to a pint of sage tea. (Dr. Faulcon, p. 103.)

AFFECTIONS OF THE URINARY ORGANS.

Catheterism.—Never use gutta percha catheters. However carefully they are used, they are liable to break, simply from the mode of their construction. (Mr. C. Hawkins, p. 194.)

Chancre.—Ninety-nine out of a hundred cases of venereal sore are better cured without mercury than with it. One of those which cannot be cured without mercury, is a sore characterized by intense induration, to which ulceration succeeds. (Mr. F. C. Skey, p. 200.)

Penis, *Cancer of.*—Employ the actual cautery to execute the amputation. The patient being put under the influence of chloroform, and wet compresses disposed round the penis, the organ is held by means of forceps, and the heated iron applied perpendicularly to the back of the penis. When the fibrous structure is divided,

the iron is to be passed into the corpora cavernosa, and so through the penis. Four or five applications will be wanted in all. (M. Bonnet, p. 199.)

Stricture.—Remember that some cases of stricture are attended with such profuse purulent or muco-purulent discharge, that we are liable to overlook the real disease and treat the case as gonorrhœa. (Mr. H. Smith, p. 190.)

[An instrument for the treatment of strictures by fluid dilatation is described by Mr. T. Machell, p. 192.]

Suppression of Urine.—Give a drop of croton oil, with a full dose of calomel and colocynth; follow this by a draught containing sulphate of magnesia, cream of tartar, and infusion of senna; and then give a large stimulating glyster, so that the bowels may be freely purged. Then cup over the loins, or bleed, if the state of the pulse admits; and give warm baths. Afterwards apply a blister to the loins, and give full doses of powdered cantharides,—a remedy which, however inapplicable theoretically, has been followed by the best results. (Dr. J. C. Hall, p. 113.)

Urethra, *Vascular Tumours of the.*—Remove them by excision or by the ligature, the latter being preferable in consequence of the serious hemorrhage which sometimes attends excision: in either case apply a powerful caustic afterwards to prevent the growth being reproduced. For very small tumours, it will be sufficient to use the actual cautery, or caustics, such as potash, nitric acid, and pernitrate of mercury: nitrate of silver is less efficient and much more painful. (Mr. H. B. Norman, p. 198.)

Uric Acid Diathesis.—Order low diet and vegetable food; employ the warm bath, and give carbonate of soda or potash, or liquor potassæ, taking care not to give them till three or four hours after a meal, otherwise they may interfere with digestion. Some vegetable acids may be given, but everything liable to generate acid in the stomach must be scrupulously avoided. (Mr. B. Cooper, p. 188.)

AFFECTIONS OF THE SKIN, &c.

Abscess.—Instead of introducing tents into abscesses, pass a stick of nitrate of silver into the wound and withdraw it immediately. Repeat this every two or three days. (M. Nonat, p. 219.)

Acne.—The disease arising from so many causes, the treatment can only be successfully conducted by taking these into account. Exclusively local, or exclusively general treatment, are equally erroneous. (Dr. T. H. Burgess, p. 207.)

Bedsores.—Apply collodion, with a view of preventing bedsores. (p. 218.)

Chilblains.—In the earliest stage use friction, either dry or with brandy or camphor. When the parts are red, swollen, and shining, but not ulcerated, rub with a solution of camphor, 4 parts, in oil of turpentine, 30 parts. When ulceration has taken place, apply the following ointment: lard, 1 oz.; liq. plumb. acet., 12 drops; thebaic extract, 3 grains; kreasote, 10 drops. (M. Ossieur, p. 215.)

Eczema.—Direct the treatment chiefly to the constitution, giving mild tonics and alteratives. Keep the parts cleanly, and when it is thought the discharge may be safely arrested, apply mild lotions containing carbonate or bicarbonate of potash, and let the bath be frequently used. If these remedies are not sufficient, give active purgatives if the patient is strong; or where these are not indicated, give sarsaparilla and hydriodate of potash; and employ lotions of nitrate of silver or bichloride of mercury. If there is inflammatory tendency in the parts, apply a few leeches behind the ears. If there is much smarting, with abundant serous exudation, give sulphuric acid internally, beginning with a small quantity, administered in barley water, and a little cold water after each dose. (Dr. T. H. Burgess, p. 204.)

In *acute* cases the constitutional treatment is most important, and must depend upon the kind of constitutional affection; the disease being usually associated with either the oxalic, lithic, or phosphatic diathesis, or with a scrofulous taint. The local treatment here is of very subordinate importance. But in *chronic* cases, the local treatment is by far the most important, and should consist in the application of a solution of ʒij. of the subcarbonate of soda in a pint and a half of water; lint saturated with this solution being applied over the affected parts, and the whole covered with oil-silk. (Dr. J. H. Bennett, p. 203.)

When this affection is connected with a scrofulous constitution, give cod-liver oil. (p. 367.)

Chronic, of the Legs.—This disease is very difficult to manage, but is best treated by the application of the vapours of iodine and sulphur, assisted during the interim of the application by bandaging from the foot upwards. The best way to apply the vapour is the following: get a tin case or a common jar, large enough to hold the limb, and place a heated iron at the bottom of the apparatus, and a grating above it, to protect the limb. Place one of the powders, about to be mentioned, on the heated iron, put the limb instantly into the bath, and cover the mouth over to prevent the vapour from escaping; and continue the bath fifteen or twenty minutes. The powders consist of, sulphur. ʒiij.; hydr. sulph. rub., ℈ij.; iodin. gr. x.; divided into six powders. In a few days the proportion of iodine may be increased. (Dr. T. H. Burgess, p. 205.)

Impetigo.—In *acute* cases, treat the particular constitutional state which may accompany the eruption; the local treatment being

unimportant. In *chronic* cases, on the other hand, be particular about the local treatment. Apply lint saturated with a solution of subcarbonate of soda (ʒ ij. to Oiss. of water,) and cover it with oiled silk. (Dr. J. H. Bennett, p. 203.)

When this affection seems connected with the scrofulous diathesis, give cod-liver oil. (p. 367.)

Lepra.—When the functions have all been got into a healthy state, and the system is free from inflammatory action, begin with Fowler's solution of arsenic, in four or five minim doses, taken thrice a day upon a full stomach; and, without increasing the dose, continue this plan steadily till the system is brought under the influence of the remedy. Continue the medicine for some time after the disappearance of the eruption. (Mr. S. Griffiths, p. 211.)

Let strong tar ointment ($\frac{1}{3}$ to $\frac{1}{4}$ of tar) be rubbed in gently thrice a day; while a tepid bath is used once or twice a week. And give Fowler's solution of arsenic, beginning with five drops daily, and increasing a drop every other day, until twelve or fifteen are reached. (M. Emery, p. 212.)

In obstinate cases of lepra, use fumigation with sulphur and iodine vapours. See "*Eczema of the Legs.*" (Dr. T. H. Burgess, p. 205.)

Pityriasis.—When recent, treat as a mildly inflammatory affection. Give gentle saline purgatives twice or thrice a week; in strong constitutions bleed occasionally, or apply half a dozen leeches to the verge of the anus; and order a bath containing four ounces of carbonate of soda, at a temperature of 80° to 90° F., according to the season, every second day. When the disease becomes chronic, use the same treatment as for psoriasis. And in all cases, whatever be the treatment found to be successful, direct it to be continued for a month or six weeks after the eruption has ceased, and the tepid baths to be continued twice or thrice a week for several months longer. (Dr. J. M. Neligan, p. 388.)

Psoriasis.—After bringing all the functions into a healthy state, and taking care that there is no inflammatory action about the system, give arsenic. Begin with four of five minims of Fowler's solution, taken thrice a day on a full stomach; and do not increase the dose, but continue it steadily till the system comes under its influence. And continue this course of arsenic for some time after the disappearance of the disease. (Mr. S. Griffiths, p 208.)

Let strong tar ointment ($\frac{1}{3}$ to $\frac{1}{4}$ of tar) be rubbed in gently thrice a day, and a tepid bath taken once or twice a week. And give arsenic, beginning with five drops of Fowler's solution (daily?) and increasing the dose by a drop every other day until twelve or fifteen are reached. If any unpleasant symptoms appear, suspend the medicine for a day or two, and then begin again with the small dose. (M. Emery, p. 212.)

Though arsenic is undoubtedly of great value in this and similar diseases, it is more efficacious when combined with iodine. When, indeed, arsenic from any cause cannot be given, iodide of potassium will be a very good substitute. In ordinary cases begin by giving one-twelfth of a grain of iodide of arsenic thrice a day, and after this has been continued for a month or six weeks, order a mixture of liq. arsen. ʒ ij.; potas. iodid., ʒ ss.; syr. simpl., ʒ ij.: and aquæ distil., ℥ vss.; a dessert-spoonful thrice a day after meals. In inveterate cases, or when this mixture has been continued in gradually increasing doses for some weeks without decided effect, add six grains of iodine to the mixture. In the local forms of the disease, besides the constitutional treatment, let iodide of sulphur ointment be applied thrice a day, the parts being washed before each application with solution of carbonate of soda (ʒ ss. ad Oj.), and then well dried. Begin with not more than eight grains of the iodide of sulphur to the ounce of wax cerate, and never increase it to more than a scruple to the ounce. (Dr. J. M. Neligan, p. 385.)

Snake-bite.—Paint the part bitten, and the whole extent of the swelling, with three or four coats of tincture of iodine, twice daily. (Dr. Whitmire, 219.)

Sting *of a Bee.*—Apply the juice (oil) of tobacco, as it is found in the mouth end of a smoked cigar, or in the reservoir of a German pipe; or apply honey, or indigo. (p. 357.)

Ulcers, *Chronic.*—Give half a grain of opium, night and morning, and apply straps and a bandage to the limb. (Mr. F. C. Skey, p. 215.)

Employ fumigation, in the following way. ℞. Sulphuris, ʒ iij.; hydr. sulph. rubr. ℈ ij.; iodinii, gr. x. M. Ft. pulv. sex. Put one of these powders upon a heated iron at the bottom of a large jar or tin case, the iron being covered by a grating. Immediately put the limb into the jar, and close it up to prevent the escape of vapour; and continue the bath for fifteen or twenty minutes. In a few days the proportion of iodine may be increased. (Mr. A. Walker, Dr. T. H. Burgess, p. 205.)

AFFECTIONS OF THE EYE AND EAR.

Cornea, *Opacities of the. Nebula.*—Apply daily a solution of nitrate of silver, made with one to three grains of the nitrate to seven drachms of aq. dest., and one drachm of vin. opii without the aromatic. Or in severe cases use the following application once or twice a day: bichloride of mercury, two grains; distilled water, an ounce. In applying these or other lotions, do not drop them into the eye, but touch the opaque spot with a camel-hair pencil *dipped* into the solution. It is often advantageous to vary the kind of application every week or two. (Dr. J. C. Hall, p. 225.)

Leucoma.—Even in the worst cases, where the central portion of the opacity is quite beyond the power of art to remove, the edges of the spot may be much improved by persevering treatment. Let a very small portion (not larger than a large shot corn) of the following ointment be put into the eye every night or every second night:—nitrate of silver, three to ten grains; solution of diacetate of lead, twenty drops; lard, one drachm. Give a drachm of the solution of bichloride of mercury, with a drachm of tincture of bark, in distilled water, twice a day: this should be given uninterruptedly for a considerable time. And apply counter-irritation behind the ears, or at the back of the neck, by blisters usually, or, if blisters are from any cause inadmissible, by setons or issues. In cases of strumous children, in whom mercury is inadmissible, use appropriate local treatment, and a course of tonics, cold-bathing, &c. (Dr. J. C. Hall, p. 225.)

Deafness.—In deafness caused by thickening of the membrana tympani, by deficiency of ceruminous secretion, or by a horny and dry condition of the auditory canal, and in many other cases, the application of glycerine will be beneficial. It is thus applied: wash the auditory passages carefully by means of cotton held between the blades of a pair of forceps adapted to the purpose, and dipped frequently in warm water, and dry them in the same manner; then apply the glycerine by the same means, passing the cotton, well soaked in it, repeatedly backwards and forwards in each meatus, and taking care to apply it to the tympanum. If a proper pair of forceps are not at hand, use a camel-hair brush. (Mr. T. H. Wakley, p. 230.)

Iritis, *Chronic.*—Great benefit has been derived from the long continued use of small doses of the bichloride of mercury, given in solution. (Dr. J. C. Hall, p. 227.)

Ophthalmia, *Granular.*—Acetate of lead is a much more efficient and less painful remedy than nitrate of silver. Apply it uniformly over the surface of the eyelids by means of a fine hair-pencil moistened with water and then dipped in the impalpable powder of acetate of lead. (M. Buys, p. 223.)

MIDWIFERY AND DISEASES OF WOMEN.

Cauliflower Excrescence *of the Os Uteri.*—As this disease is, in all probability, not of a malignant nature, we may hope by its removal to effect a permanent cure. It is necessary, however, that the removal should be complete; and the safest and most efficacious mode of accomplishing this end is by free excision of the affected lip, or, if necessary, of the entire neck of the uterus. (Dr. E. Watson, p. 303.)

Chloroform should be given in all turning, forceps, and crotchet cases, but especially the first. When there is no organic dis-

ease present, there is little danger to be apprehended from giving it in any case of labour, especially when it is remembered that immunity from pain may be secured, without giving the chloroform to such a degree as to take away reason or consciousness. (Dr. Denham, p. 299.)

Chlorosis.—Salts of manganese are recommended, especially in those cases which are not much benefited by iron. The sulphate, carbonate, or phosphate of manganese may be given in the form of pill, to the extent of three or four grains daily; or the phosphate or iodide may be given in syrup, according to the formulæ given at p. 371. (Mr. Hannon, p. 25.)

Climacteric Disease.—Remove any obvious cause of uterine irritation, by appropriate treatment. ‖Use moderate depletion from the uterus, by incisions made into the os, or still better, by the application of three or four leeches to the os. And use cold hip-baths, or cold injections into the vagina or rectum. If there are any dysmenorrheal symptoms, use injections with decoction of poppies and laudanum, into the rectum and vagina; if there is menorrhagia, let alum baths (℥xvj. of alum to each gallon of water, at temp. 98°) be used in the intervals between the periods. Keep the bowels lax, but avoid drastics, and especially aloes; and promote the alvine, renal, hepatic, and cutaneous secretions. Let the diet be light and nutritious, with very little wine and no malt liquor or spirits. Let the clothing be warm, and in winter let flannel jackets and drawers be worn. And for the severe symptoms of nervous irritation, give moderate doses of sulphuric ether and valerian. Sulphuric ether is at this period a more decided sedative upon the female constitution, than either morphia, opium, or hyosciamus. (Dr. W. Tyler Smith, p. 289.)

Dysmenorrhœa.—The administration of lemon-juice has been found very useful in some obstinate cases. (Mr. Headland, p. 32.)

Hemorrhage, *Post-Partum*.—In extreme cases pass one hand into the uterus; introduce the pipe of an enema apparatus by the side of the arm a little way into the vagina, and inject suddenly ice-cold water, which will pass up into the uterus. By the ordinary plan of injecting the vagina, we are not sure that the injection enters the uterus; but by having one hand introduced into this organ, we learn whether it is really injected, and we ascertain also the effects of the injection. This method is very much better than that of introducing a bladder into the uterus and then distending the former; as it is the influence of the cold, and not the pressure, that we want to employ. (Dr. W. Tyler Smith, p. 240.)

Inversio Uteri.—When the organ is not affected with malignant disease, and no other special contra-indication exists, and the patient's health is suffering, extirpation of the womb is a proper proceeding. In performing the operation, tie a flat tape, half-an-inch wide, round the inverted vagina as high up as possible. This, when tightened, acts as a tourniquet, without injuriously

pressing on the parts,—it allows sutures to be easily inserted,—and, if retained on for some time after the operation, it keeps the parts closely in apposition, and prevents dragging upon the sutures. (Dr. C. H. Higgins, p. 281.)

Labour, *Protracted.*—Never be anxious to promote hasty delivery, and use all operative measures as seldom as it is possible. The late Dr. J. Clarke, of Dublin, who used the forceps only once in 3847 labours, was singularly successful in saving the lives of both mothers and children. (Dr. R. Collins, p. 236.)

Mammary Abscess.—After opening an abscess of the breast, instead of introducing a tent, pass a stick of nitrate of silver into the wound and withdraw it immediately. This is much less painful than a tent, and is quite as efficacious in preventing closure of the opening, and in inducing the contraction of the abscess. It must be repeated every two or three days. (M. Nonat, p. 219.)

Menorrhagia.—Give oxide of silver, in doses of half a grain or a grain three times a day, combined with a small quantity of morphia or opium. It is almost a specific for those cases in which there is no organic lesion. If there are high and inflammatory symptoms, these must be subdued before beginning with the remedy. Oxide of silver blackens the stools. (Dr. J. J. Thweat, p. 282.)

Some cases which resist ordinary treatment, will yield to brisk and frequent purgation, with small intermediate doses of calomel. (Mr. G. Corfe, p. 287.)

Os Uteri, *Application of Leeches to the.*—An instrument by means of which this can be very easily accomplished, is described by (Mr. W. Clegg, p. 273.)

Pelvic Abscess.—The abscess must be opened where it points. If in the vagina, an incision may be made there; but if in the hypogastric region, adhesion of the two surfaces of the peritoneum must be obtained before opening the abscess. In order to effect this adhesion, apply a portion of potassa fusa cum calce, made into a paste with alcohol, to that part of the abdominal parietes where it is wished to make the opening. When, by the action of the caustic, after one or more applications, the seat of the fluctuation is nearly reached, and adhesion is shown to have taken place by the impossibility of moving the abdominal parietes over the tumour, then make an incision in the centre of the sore. Or, instead of using the caustic, make an incision through only a portion of the thickness of the abdominal parietes, and then apply linseed poultices; the pus will frequently find an exit through the sore. After the abscess is opened, use injections of tepid water, to remove fœtid secretions, and to prevent the admission of air, by keeping the cavity full of liquid. (Dr. E. J. Tilt, p. 286.)

Prolapsus Uteri.—[A new elastic truss or supporter is described by Dr. C. Ritchie, p. 293.]

Sterility.—The greatest discrimination is required in the use of mechanical treatment; both because it is not easy to distinguish cases in which there is really mechanical obstruction, from perfectly natural conditions, and also because the operations for the relief of such obstructions are not without danger. *Strictures of the os or cervix* sufficient to prevent impregnation are very rarely met with; and the attempt at mechanical dilatation has been followed by a fatal result. The treatment of *malposition of the uterus* supposed to be the cause of sterility, has also caused death. Lastly, *obstructions in the fallopian tubes* are probably never, when they do occur, of a kind which could be overcome by catheterism; and as this operation will be always extremely difficult, and never without danger, its performance appears to be altogether indefensible. (Dr. Oldham, p. 268.)

Ulceration *of Os Uteri.*—In cases of congestion and erosion, or superficial ulceration, commence with a slight application of nitric acid, or acid nitrate of mercury. Afterwards paint the entire cervix one or twice a week, but not oftener, with the following: R. Iodin., ʒj.; potass. hydriod., ʒij.; aquæ destil., ʒij.; sp. vini, ʒij. Solve. Continue the use of this application perseveringly, until the congestion is relieved, and then do not discontinue it abruptly, but by degrees. (Dr. Churchill, p. 273.)

MISCELLANEA.

Amputation *of the Penis.*—Is best performed with the actual cautery. The patient being put under the influence of chloroform, and wet compresses disposed round the penis, this organ is put on the stretch, and the cautery applied to its back. Four or five applications of the hot iron will be wanted to complete the section. (M. Bonnet, p. 199.)

Anæsthetics.—Bromoform, Dutch liquid, and common coal gas, are safe and powerful anæsthetics; and the latter, as being cheap, deserves a further trial. Sulphuric ether should be given when there is much constitutional debility, or where the operation is likely to be long and tedious, or where it is likely to produce considerable shock and depression. Chloroform, though very agreeable and powerful, requires more care in its administration than has usually been employed. The principal precautions to attend to in its administration are:—not to give too much at once, and not to let the temperature be too high,—to take care that the air can pass freely in and out of the lungs,—not to give the vapour in too concentrated a form at first,—to withdraw the anæsthetic as soon as there are indications of unconsciousness and insensi-

bility being produced,—and then to give the vapour much more diluted, so as just to keep up the condition of insensibility. (Mr. Nunneley, p. 307, &c.)

Dutch liquid possesses no advantages over chloroform but such as are connected with its slower action and more persistent effects. It is, therefore, only on a few occasions that its use is to be preferred. (Dr. Snow, p. 379.)

Remedies for an Overdose.—Remove the anæsthetic: dash a small quantity of water upon the face and chest, possibly alternating with heat to the latter; use moderate interrupted compression of the lungs, so as to change the air in the lungs, and so get rid of the vapour there remaining; or, with the same intention, blow a stream of air, not too forcibly, through a small tube into the larynx for a minute or two. It is important, in the present state of our knowledge, not to attempt to do too much. (Mr. Nunneley, p. 315.)

Mode of Inhalation.—Use a large, moderately fine, linen handkerchief, folded first into quarter size, and then into a funnel shape. (Mr. Nunneley, p. 312.)

Use a piece of spongio-piline sufficiently large to cover the mouth and nostrils, made into a hollow cone, with a few ribs of whalebone fastened on the outside, to prevent it from collapsing, and over these a covering of fine leather. (Dr. Denham, p. 299.)

Always use an apparatus by which the amount of air can be regulated exactly. (Dr. J. Snow, p. 317.)

Baths, *Vapour.*—[A new vapour bath, by means of which simple or medicated aqueous vapours may be much more easily applied than by the common one, is described by Dr. Golding Bird, p. 347.]

Hot-air.—A convenient and efficient hot-air bath, may be thus extemporized:—the patient being in bed, fasten a blanket loosely round the body; place two chairs upon the bed, so that their backs form an arch over the person, tie them side by side, and over them throw several blankets. A spirit lamp held beneath the foot for a few minutes will raise the temperature to 130° or more. If a spirit lamp is not at hand, one may be made out of a tea-pot, letting the wick pass through the spout. (Mr. W. N. Spong, p. 408.)

Blisters.—Smear the surface to be vesicated with "cantharidal collodion." This will be all that is required. (M. Hisch, p. 374.)

Injections.—[A syringe with a spiral spring, presenting many advantages over the common syringe, is described by Mr. T. H. Wakley, p. 343.]

Issues.—May be thus conveniently made by means of galvanism. Get a piece of perforated zinc fastened or rivetted to a sixpence, shilling, or half-crown. Place this on the spot where the issue

is to be made, with the zinc surface next to the skin, and cover it with a piece of spongio-piline moistened with salt and water. Moisten the spongio-piline with salt and water every twelve hours. An eschar will be thus formed in about twelve days, or, if the cuticle is removed before applying the little battery, the slough will be formed in from four to six days, but more irritation is created than by the former method. (Dr. T. Smith, p. 342.)

LEECHES, *Application of, to the Os Uteri.*—[A little instrument, by the help of which this may be most efficiently and readily effected, is described by Mr. W. Clegg, p. 273.]

POISONING.—Fresh animal charcoal, when used in sufficient quantity, has the property of withdrawing from their solutions most, if not all known vegetable and animal poisonous principles, and some mineral poisons. It should therefore be immediately administered in all cases; while in the case of mineral poisons, the usual antidotes should also be given. (Dr. B. H. Rand, p. 377.)

By Arsenic.—Besides the usual means for removing the poison from the stomach, give calcined magnesia, in doses of a drachm every hour. (Dr. E. Bissel, p. 377.)

SHOULDER-JOINT, *Disease of.*—A peculiar form of chronic arthritis sometimes affects this joint, leading to atrophy both of the soft parts and the bones. The treatment of these cases will vary according to the stage of the disease. During the early and inflammatory period, use repeated leechings and small cuppings, mercurial and belladonna ointment, and emollient and resolvent poultices. During the second stage, that of muscular atrophy, apply large blisters, moxas, or the cautery before and behind the joints, and use baths, with local stimulants and tonic douches, of plain, sulphureous, or aromatic waters. Lastly, when the bones become affected, lay aside the antiphlogistic and derivative treatment, and employ friction, especially with oil of turpentine; use also passive motion, taking care, however, not to excite fresh inflammation by doing this too soon, and not to run the risk of anchylosis, by delaying it too long. (M. Velpeau, p. 120.)

STAINS *from Nitrate of Silver.*—Wet the linen, &c., in a solution of bichloride of mercury, (1 part to 31) rubbing it well, and then wash in cold water. (M. Martinenq, p. 371.)

Brush over the stains on the linen, skin, nails, or teeth, with a solution of cyanide of potassium, eight or ten grains to ℥j. of distilled water. If the stains are superficial, one or two applications will suffice; if deep, several will be required. (Mr. Collins p. 177.)

TENOTOMY.—[For minute directions as to the performance of this operation, see p. 125.]

VENTILATION.—The most efficient ventilation can be obtained by a very simple apparatus, consisting of an inverted syphon, with unequal legs; the air will pass down the shorter, and be discharged by the longer leg. (Dr. Chowne, p. 357.)

ASIATIC CHOLERA.

PREMONITORY SYMPTOMS.*—On the first appearance of premonitory symptoms, give a Seidlitz powder, or if the patient has a sense of sinking, without diarrhœa, add three drachms of Epsom salts to the Seidlitz; and, when it begins to act, let the patient drink freely of thin beef-tea, well salted. If there is vomiting, apply a sinapism to the epigastrium: if intense thirst, allow Seltzer, soda, or plain water, ad libitum. (Dr. Stevens, p. 392.)

When the purging is not restrainable by opium, kino, and chalk, in full doses, give a pill containing a grain of nitrate of silver, and a quarter of a grain of opium, every hour, to the extent of five or six doses, or even more, if necessary. (Mr. G. Ross, p. 421.)

Give opium fearlessly, but judiciously, unless collapse is approaching, *when it must not be given.* To children give the following:—ext. hæmatox., ℈ij.; vin. ipec, ʒi.; vin. opii, ♏xx.; aq. cinnam. ℥iv. Of this give ʒj. after every motion to a child six months old; ʒij. to a child of one year; and ℥ss. to one of two years. Feed the child on milk mixed with a little lime-water, keep it in bed, and cover the abdomen with hot linseed-meal poultices. (*Adopted at* Guy's Hospital, p. 401.)

Nitro-hydrochloric acid has been useful, when the stomach would bear nothing else. (*Adopted at* St. Thomas's Hospital, p. 402.)

Give the following draught every hour, or every two hours:—strong nitric acid, two and a half to eight minims; tincture of opium, four to eight minims; syrup of saffron, a drachm; water, an ounce and a half. (Mr. W. Batten, p. 417.)

Give two table-spoonsful of the following mixture every two hours, and no astringents whatever: ℞. Kreasot. gtt. xvj.; mist. acaciæ, ℥ss.; sp. ammon. co., sp. camph., āā ʒij.; ether. chlor. ʒiij.; aquæ ℥viss. M. (Mr. C. N. Spinks, p. 432.)

Give syrup of pernitrate of iron, (which is made with clean iron wire ʒvj.; nitric acid ℥iss.; water ℥viij.; sugar ℥xiv.) To each ounce of this syrup, add tinct. opii ʒj.; sp. camph., gtt. xxx.; and give sixteen or twenty drops in a little water every hour till relief is obtained. If there is very much pain, add tinct. capsic. ʒi. to the ounce of syrup, and give the dose in a little burnt brandy. (Dr. T. Reynolds, p. 439.)

* For the convenience of the reader we have classified the remedies here recommended, into those applicable to the "*Premonitory Symptoms,*" and those applicable to "*Developed Cholera.*" It is necessary to observe, however, with regard to *some* of the plans of treatment which we have placed under the latter head, that we are unable to state to what precise period of the disease they refer. Considerable variety occurs in the nomenclature used by different writers, and in the communications sent to the journals during the late epidemic, written, no doubt, amidst the hurry of business, the terms "premonitory symptoms," "premonitory diarrhœa," "first stage," "early stages," appear to have been used with considerable laxity.

For information respecting some modes of treatment not here described, the reader is referred to Retrospect, Vol. XVIII. p. 465.

Give the liq. chlorinii of the Edinburgh Pharmacopœia, in doses of ʒi. or ʒiss. in water, every hour, until the symptoms are relieved. (Mr. C. W. Mann, p. 440.)

Give from ♏v. to ♏xv. of chloroform internally, every hour or two, mixed with mucilage; and let the patient drink freely of milk and cold water, with a little sesquicarbonate of soda in it. (Dr. J. Davies, p. 439.)

Developed Cholera.—In the stage of collapse, whether it is just commencing, or far advanced, give a pill containing two grains of calomel every ten minutes, with one or at most two drops of laudanum in a little water. Continue the calomel at the same intervals for five, six, or more hours, only lessening the dose and widening the intervals, as the coldness and lividity subside, and discontinuing it when the pulse and skin become natural. After forty or fifty drops of laudanum have been taken, give it only at wide intervals; indeed it might probably be done without at all. Give no opiates in excess, and no stimulants whether alcoholic or pharmaceutic; and employ no other auxiliary means whatever. (Dr. J. Ayre, p. 406.)

Give two grains of calomel, and two drops of laudanum every ten minutes; employ a hot-air bath, (*see* "Bath, *Hot-air*",) and place a drachm of strong mercurial ointment in each axilla; and give ten drops of chloroform with every alternate dose of the calomel. Omit the opiate and chloroform, and give the calomel at longer intervals, when reaction commences. The readiest way of administering the calomel, is to put two drachms on a sheet of paper, and divide into sixty parts; one of these to be given for a dose, from the point of a table-knife. And as to the opiate, bale out any number of spoonsful of water, and add twice the number of drops of laudanum: then by giving a spoonful of the mixture, using the same spoon, exactly two drops of laudanum will be given. (Mr. W. N. Spong, p. 408.)

Place on the tongue two grains of calomel mixed with a little sugar, and let it be washed down by an effervescing draught. If it is rejected, repeat the dose immediately; if retained, repeat every five or ten minutes till reaction commences, when the period between each dose may be gradually extended to an hour or longer. Large doses at long intervals are of no use. (Dr. P. Niddrie, p. 427.)

Give twenty or thirty grains of calomel at once; repeat it in ten minutes; and employ such auxiliary means as warmth, friction, and gentle stimulants. (Anon, p. 410.)

Complete recovery from the state of collapse has followed the administration of a scruple of calomel every hour for three times. (Dr. J. C. Badeley, p 409.)

The calomel treatment is of no use whatever. (Dr. Golding Bird, p. 412.)

Calomel is of no use. Before collapse occurs, give a large dose of solid opium, followed by astringents, ammonia, and opium in a fluid form, and an enema composed of a little warm starch, with a full dose of laudanum. When collapse has set in, let cold fluids be taken freely, with a moderate quantity of ether, ammonia, chloric ether, or turpentine; and sponge the surface of the body, and wrap it in warm blankets. (Dr. Hughes, p. 411.)

Give a powder composed of sod. sesquicarb. ℈j.; sodii chlorid. ʒj.; potas. chlorat. gr. vij; dissolved in half a tumbler of water, every hour, half hour, or fifteen minutes, until the circulation is completely restored; then lengthen the intervals between the doses, and when reaction is completely established, leave the medicine off by degrees. Relieve the cramp by rubbing the extremities with hot flannel, and applying sinapisms. Keep a large fire in the room, day and night, and remove all the dejections, &c., immediately they are passed. Seltzer, or cold water, may be allowed *ad libitum*, and occasionally a strong infusion of green tea. But do not let the patient have any solid food for at least five days after recovery from a state of collapse.

In extreme cases, the dose of chloride of sodium may be increased to two drachms, or even more And in still worse cases, where there is collapse, but neither vomiting nor purging, give an ounce of chloride of sodium, with half a drachm of chlorate of potash, in cold water; and repeat it, if necessary, every half hour, till three doses have been given. Then, if reaction ensues, keep it up by the smaller doses first mentioned: if it does not, inject a saline fluid either into the intestines or the veins.

If there is great irritability of stomach, apply a large sinapism immediately, inject a dilute solution of common salt, at a temperature of about 100° into the bowels, and give the common soda powders with excess of alkali. When the vomiting abates, give the saline powders, without the common salt, and with rather more chlorate of potash, in a very small quantity of water; or give the chlorate of potash alone, in ten-grain doses.

Above all, do not give opium, or cajeput oil, or use extreme cold, or apply galvanism. (Dr. Stevens, and many others, p. 393, &c.)

In the stage of collapse, give small and frequent doses of chlorate of potash, bicarbonate of soda, and hydrochloric ether, largely diluted in camphor water, or with the patient's drink; or aqua chlorinii with hydrochloric ether, in the same vehicle; and persist in giving them though the vomiting continue. If reaction follow, give calomel and camphor; and in this stage, if the kidneys do not act, give diuretic salts, and apply embrocations with turpentine to the loins, or give an enema containing a little spirit of turpentine. (Dr. J. Copland, p. 403.)

Use saline injections in the stage of collapse. A quart of water at 98° Fahr., with a drachm of common salt and half a

drachm of sulphate of potash, may be injected into a vein in the arm. As soon as the pulse is established, envelope the patient in a sheet wrung out of cold water, cover him with several layers of blankets, and apply hot bottles to the epigastrium and calves. After the profuse perspiration which this occasions has continued for, say, upwards of an hour, remove the blankets and sheet, and put the patient in hot blankets. If any fever follows, give calomel and opium. (Mr. H. Howlett, p. 398.)

Inject from 40 to 60 oz. of a saline fluid; waiting after a portion of it has been injected in order to see the effects. A solution like the following may be used: chlorate of potash, fifteen grains; carbonate of soda, one scruple; common salt, two drachms; water, forty-eight ounces. (Dr. Marsden, p. 399.)

Transfusion of blood ought to be employed. If sufficient blood cannot be obtained, inject what blood you can get, first, and afterwards water containing saline substances. (Dr. C. H. F. Routh, p. 149.)

Inject into the veins six ounces of water at blood-heat, with thirty drops of a solution of morphia of the following strength:—crystallized muriate of morphia, five grains; pure hot water, one ounce. When the discharges have been checked by the morphia, give calomel by the mouth, or introduce mercury into the veins in one of the following forms:—one grain of corrosive sublimate, five grains of iodide of potassium, an ounce of warm water; or crystallized cyanide of mercury, one grain, warm water, one ounce. A fluid drachm of either of these solutions may be injected mixed with six ounces of warm water, and repeated if required. (Dr. R. M'Gregor, p. 427.)

In the stage of collapse, wrap the patient completely, except the head, in a sheet wrung out of boiling water, cover him with eight or ten blankets, and give shortly a little weak brandy and water. After keeping him in the blankets an hour, put him into a clean warm bed. Then give nitro-hydrochloric acid with syrup of poppies, either in water or infusion of catechu, every hour or so, if the stomach will bear it. (*Adopted at* St. Thomas's Hospital, p. 402.)

Wrap the patient in a blanket which has been wrung out of the hottest water, and sprinkled with a handful or two of salt, and put four or five blankets closely round him; this may require renewing in an hour or two. Give cold water *ad libitum*, and beef-tea frequently, but no medicine, except there is much sickness, when a little carbonate of ammonia, and soda, and hydrocyanic acid may be given. (Mr. Garrington, p. 416.)

Though the wet sheet, when fairly tried at the London Hospital, had the effect of producing reaction, yet all the patients died. (p. 430.)

Give ice internally in considerable quantities, in the form of a jelly made by constantly stirring water while freezing. The water should have a little common salt added to it, the nauseous taste being covered, if required, by a little lemon-juice. (Dr. J. Arnott. p. 433.)

Administer iced water freely; sponge the whole surface of the body with cold water, and then wrap the patient in a cold wet sheet, and cover with blankets. (Anon, p. 434.)

Give an emetic of sulphate of zinc, and repeat it quickly. If it is not followed by reaction, use the warm bath, apply sinapisms to the epigastrium, and, an hour after the emetic, begin to give an effervescing solution of citrate of soda every two or three hours. Give ice and iced water to allay the thirst, and give enemata of beef-tea, with or without brandy. Stimulants and astringents are both useless. (*Adopted at* St. Bartholomew's Hospital, p. 402.)

Begin by giving an emetic of twenty-five grains of ipecacuanha. Afterwards give a dose of ether, opium, and chloroform. (Mr. Greenwood, p. 437.)

Avoid any quantity of opium, and avoid also the reckless use of alcoholic stimulants. Cover the limbs with cloths wrung out of warm water, and cover up with blankets; allow diluents to be taken copiously; give chloroform, in doses of ten minims, frequently repeated. In decided collapse, *small* doses of calomel and opium have been given; an emetic also has roused the patient greatly; but after all, the less that is done in this stage, the better. (*Adopted at* Guy's Hospital, p. 401.)

Give chloroform internally, in doses of from ♏v. to ♏xv. every hour or two, mixed with mucilage. Let the patient drink freely of milk and cold water, with a little carbonate of soda; and let the warm bath, with a large quantity of common salt in it, be frequently used. (Dr. John Davies, p. 439.)

Chloroform was tried at the London Hospital, given both by the stomach and by inhalation, but all the patients died. (p. 430.)

Give a pill containing a grain of nitrate of silver and a quarter of a grain of opium, every hour, to the extent of five or six doses, or even more if necessary. Or, if the evacuations are very profuse, give two or three pills at a dose, and repeat them until the evacuations are wholly or partially arrested. (Mr. G. Ross, p. 421.)

Give matico, in the following manner:—Take of tinct. of matico, as strong as it can be prepared, from ʒij. to ℥ss.; tinct. of opium, ʒss. to ʒi.; sp. of camphor, ♏xv. to ♏xx.; water, ℥ss. M. Give a fourth part whenever the sickness or purging comes on. Two doses generally suffice to arrest the disease; but if there is any return, it must be given again. (Mr. T. Slipper, p. 430.)

Give kreasote, according to the following formula: R. Kreasot. ♏xxiv.; mist. acaciæ, ℥ss.; sp. ammon. co., sp. camph., āā ʒij.; ether. chlor. ʒiij.; aquæ, ℥viiss. M. Cap. ℥i. omni horâ. (Mr. C. N. Spinks, p. 432.)

In slighter cases, in the more tractable stages of the disease, and in other cases as an adjunct, give a draught containing from two and a half to eight minims of strong nitric acid, four to eight minims of laudanum, a drachm of syrup of saffron, and an ounce and a half of water, every hour or two. But in confirmed forms of the disease, give phosphorus, in doses of one grain made into a pill with white wax. It may be necessary to give one of these pills at intervals of an hour or two, for two or three times, and afterwards a dose of the acid mixture. (Mr. W. Batten, p. 417.)

Give the liq. chlorinii of the Edinburgh Pharmacopœia, in doses of ʒi. to ʒiss., diluted with water, every hour till the symptoms are relieved. (Mr. C. W. Mann, p. 440.)

At an early period, give six grains of quinine, four of calomel, and two of opium, followed by a wineglass of brandy diluted with a little warm water. If given within the first hour or two of seizure, this will generally check the disease. (Anon. p. 427.)

Employ galvanism, placing one pole of the machine over the heart, and the other over the region of the solar plexus. (Dr. Peacock, p. 432.)

Transmit electro-magnetic shocks, so arranged as to imitate the frequency of the respiratory movements, or not more than twenty-five in the minute, from the nape of the neck to the upper part of the epigastrium. (Mr. Blacklock, p. 433.)

Rub the whole region of the spine with a solution of croton oil in turpentine,—one drachm to the ounce,—for twenty minutes, and then apply a hot and moist bran poultice, contained in a bag, large enough to cover the whole back. (Mr. Reeves, p. 416.)

Let warm water be plentifully injected into the bladder. (M. Piorry, p. 440.)

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TO

THE 17TH, 18TH, 19TH, AND 20TH VOLUMES,

OF

THE RETROSPECT OF PRACTICAL MEDICINE,

POINTING OUT THE PARTICULAR VOLUME AND ARTICLE IN WHICH INFORMATION ON ANY GIVEN SUBJECT MAY BE REQUIRED.

☞ Only the Volumes and Articles are referred to, and not the Pages.

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